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SOME EXPERIMENTS ON THE COLOR PERCEPTIONS OF AN INFANT AND THEIR INTERPRETATION.

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While observing the development of a normal healthy infant, I became convinced that her interest in colors, and probably her perception of them, developed during the latter part of the sixth month. The ability to grasp objects was not gained until the beginning of the fifth month. During the latter part of the fifth and early part of the sixth month, I made several rough attempts to discover whether she displayed any preference for bright colored objects, but could detect none. She had two celluloid rattles, alike except for color. One was a dull blue, and the other a brilliant rose pink. The brightness difference was slightly in favor of the blue. When the two rattles were held out for her to grasp, she took the easier one if there was any difference in the difficulty of obtaining them. If no such difference existed, her choice seemed to depend on chance. About the middle of the sixth month I thought I noticed a dawning preference for the bright pink rattle. By the end of the month, the pink rattle was so decidedly the favorite that she would reach for it when it was placed behind other toys, overlooking the blue one entirely. The color preference in this case seemed so marked that I was tempted to try a series of tests to corroborate my observation of color vision at so early a period.

The method to be used in making the tests was suggested by the behavior of the infant herself. She seemed to be passing through a stage of sense comparison. I frequently saw her looking back and forth from one to the other of two similar

objects — two faces, two chandeliers, or what not. It occurred to me accordingly to try the method of paired comparison, rather than that used by Baldwin with an older child, that of recording the number of times the child reached, or failed to reach, for a colored paper. My preliminary tests convinced me that the method was applicable. When offered two pieces of colored paper of the same size and shape the child looked back and forth from one to the other, and then grasped one of them, often with a comical appearance of mature deliberation. After grasping the paper, she turned it back and forth, looking carefully at both sides.

In making the tests, the usual precautions for securing favorable conditions were observed. Tests were made only when the child was feeling well and rested; and when the light was good. The child was placed sitting in as easy a position as possible, with both arms free to move. The colors were presented in the form of discs of colored paper four and one half inches in diameter. In each test two discs laid side by side were moved up to the child directly in front of her. Care was taken to see that the two were equally illuminated. The tests were made in series of ten, or in a few cases twelve, choices, half in each of the two positions. The influence of position, and of the hand used, was thus eliminated from the results, though both factors were recorded, and will be reported upon in a separate communication. The background upon which the discs were laid was a medium gray in all cases except those in which one of the discs to be compared was gray. In those cases the background was white.

There are two sources of error to be considered. The first one is the imperfect eye-hand coördination of the child. Occasionally it was evident that while intent on one piece of paper, she grasped the other by mistake. When this occurred, she usually held the paper a short time, with her eyes still on the other one, and then dropped it for a second trial for the preferred color. Sometimes the feeling of the first disc in her hand diverted her attention to the new sensation, and made her forget her original intention. It was easy to tell from observing the child whether she had grasped the paper she intended to take

or not. The mistakes were not of frequent occurrence, and were excluded from the results if there was the slightest ambiguity of interpretation. This source of error, therefore, seemed to me negligible. The other difficulty was somewhat more serious. It was due to the fact that the series of discs with which I began the tests was not of uniform texture. Some of them were the Hering tissue paper discs, some were the later washed papers of Hering, and some were from the Milton Bradley papers. The black was the black velvet paper of the Hering series. I soon saw that the rattle of the tissue paper, and the rough feeling of the black velvet paper were interesting to the child, and associations which began to influence choice were formed with the black velvet paper, and possibly with the tissue papers. As soon as I had noticed the fact, I made a complete series of discs from the Milton Bradley papers, and the greater part of the tests were made with these.[1]

The tables of results show the comparisons made. Each of the four colors blue, yellow, red and green was compared with each of the other three colors, and with black, white and medium gray. The tables of results fail to show the numbers demanded by uniform series of ten or twelve, because it often happened that some tests had to be discarded for various reasons, and in a few cases series were not completed because of the introduction of some disturbing element. The number of series for the various pairs is also quite uneven. The experimenter intended to complete the series which are brief, but the untimely failure of the method, about to be chronicled, made it impossible.

The experiments were begun when the child was just six months old, and were continued for a month. By the end of that time she seemed to have passed beyond the intense interest in mere sense comparison which had dominated at the start. The act of grasping, which at first had been a mere means for obtaining clearer sense impressions, had developed into the ability to manipulate objects, and that became her one desire. Instead of comparing the discs, and then carefully and laboriously grasping one or the other, she grabbed at them without

[1] I am indebted for all the discs to Professor James R. Angell and Professor John B. Watson, of the University of Chicago.

appearing to care which one she got, and often took one in each hand, a feat which had been very difficult in the early tests. When she got the discs, her whole desire was to crumple and shake them, not to look at them as at first. When this stage was reached, the tests were of course discontinued, but a sufficient mass of material had been collected to throw some light on the question of the existence of color vision at so early a period, and even on the order of preference of the four primary colors.

TABLE I.

RED.

yellow	blue	green	black	white	gray
15–17	14–4	27–5	11–4	5–2	17–3

YELLOW.

red	blue	green	black	white	gray
17–15	12–12	13–7	4–6	15–5	14–6

BLUE.

red	yellow	green	black	white	gray
4–14	12–12	19–12	6–3	8–2	7–3

GREEN.

red	yellow	blue	black	white	gray
5–27	7–13	12–19	7–4	6–4	10–10

Black, 17; white, 3.

In the tables of results, the first one of each pair of numbers indicates the number of choices of the color heading the table, and the second, of the color heading the section, when those two colors were compared. Thus in the table headed red, and he section headed blue, the first number, 14, shows that when red and blue were compared, red was chosen 14 times; and the second number, 4, that under the same circumstances blue was chosen 4 times. The total number of comparisons made between the two colors is thus indicated by the sum of the two numbers in the section.

The conclusions which it seems to me can fairly be drawn from the tables of results are as follows. The child perceived

red, blue and yellow as colors. It is uncertain whether or not she perceived green as a color. Her preference for red, and her indifference to green are striking. Blue and yellow occupy an intermediate place, with yellow somewhat in the lead, though no stress can be laid on the difference.

Let us consider first the outcome of the comparison of each of the colors with the colorless discs. Out of 42 choices in which red and a colorless disc were compared, 33 are for red and 9 for the colorless discs; or red 79 per cent. and colorless discs 21 per cent. There are 50 choices in which yellow and the colorless discs were compared. Of these 33 are for the yellow, and 17 for the colorless discs; or yellow 66 per cent., colorless discs 34 per cent. There are only 29 cases in which blue was compared with the black-white series. Of these 21 choices are for the blue and only 8 for the colorless discs, or blue 72 per cent., colorless discs 28 per cent. Green and the black-white series were compared 41 times. Of these 23 choices are for the green, and 18 for the colorless discs, or green 56 per cent., colorless discs 44 per cent.

If estimated by the preponderance of choice of colored over uncolored papers, the order is therefore red 79 per cent., blue 72 per cent., yellow 66 per cent., and green 56 per cent. The percentage of choices for green (56) is too small to serve as the basis of any conclusion. The others are large enough, in my estimation, to justify the inference of color vision.

In estimating the order of preference of the colors by means of the comparison of one color with the others, the irregularities to be expected in such a series of tests are disturbing, but the general trend of the tables shows differences in choice marked enough to be significant. For instance, in the direct comparison of red and yellow, the choices for yellow are slightly in excess, and yet the preference for red when compared with the other colors is so much greater than that for yellow, that it seems clear that red is the preferred color. When the preference for color is estimated by a comparison of the number of choices for each color with the number for the other three with which it was compared, the outcome is as follows. Red was compared with the other colors 83 times. Of

these 56 choices are for the red and 26 for the other colors; or red 68 per cent., other colors 32 per cent. Yellow and the other colors were compared 76 times, of which 42 choices are for the yellow, and 34 for the other colors, or yellow 55 per cent., other colors 45 per cent. There were 73 comparisons of blue with the other colors, of which 35 are for the blue and 38 for the other colors, or blue 48 per cent., other colors 52 per cent. Green was compared with the other colors 83 times, of which 24 choices are for the green and 59 for the other colors; or green 29 per cent., other colors 71 per cent. In this case the order of preference is accordingly red, yellow, blue and green.

When the total series is summed up by estimating the percentage of choices for each of the colors from the total number of pairs in which each one appears, the result is as follows. Out of 124 pairs in which red is one member, red is chosen 89 times, or 72 per cent. Out of 126 pairs in which yellow is one member, yellow is chosen 75 times, or 60 per cent. Out of 102 choices in which blue is one member, blue is chosen 56 times, or 54 per cent.; and out of 124 choices in which green is a member, green is chosen 47 times, or 38 per cent.

The positions of blue and yellow when the preference for color is inferred from the comparison of the colors with the uncolored discs are not the same as when the basis of judgment is the comparison of each color with the other colors, or with all the other discs used. My reason for thinking the latter two formulations the more significant is that the number of comparisons of blue with the uncolored discs happens to be much smaller than the corresponding series for the other colors. Moreover, as I shall point out presently, the child had a preference for black, which, in so far as it tended to influence the choice of colors, would enhance the value of blue fictitiously.

The question of the influence of brightness differences on color choice must of course be considered. In the series of papers used, yellow is the brightest color, green next, red third, and blue darkest. It is obvious that the child's color preferences do not coincide with the brightness series, either ascending or descending. There is very little difference in her

liking for yellow and for blue, the lightest and the darkest colors; and both hold a place intermediate between red and green, which are the intermediate members in the brightness scale. If the order of preference for the brightness is figured from the tables of results in the same way as that for the colors, it appears that black ranks first, gray next and white last. Out of 45 choices in which black was a member, it is preferred 17 times, or 38 per cent. Gray was taken 22 times out of 70 choices, or 31 per cent. White was chosen 13 times out of 47 presentations, or 28 per cent. The preference for black is further shown by a series of choices between black and white on a medium gray ground. Out of 20 presentations, black was chosen 17 times, or 85 per cent. The child's interest in black had been noticed before the experiments were begun. Black dresses, and still more black hats, aroused her enthusiasm, and she displayed a passion for black shoes. The only explanation of the fact I could make was to trace it back to an incident which had happened about six weeks before the beginning of the experiments. The child was sitting in my lap with her back toward me; and with the intention of keeping her amused, I was putting up first one foot and then the other rhythmically. She was very quiet, and I was not conscious that a deep impression was being made until a friend who was sitting in the room exclaimed that the infant was very much frightened. She was staring at the appearing and disappearing feet with every expression of intense fear on her face. I at once tried to soothe her and show her what the object was. She is not subject to fears, in fact that was the only instance of it during the first year. From that time on for several months she displayed an intense interest in black shoes, and secondarily in any black object. When placed on the floor at our feet with toys about her, she invariably neglected the toys to reach for our shoes. She cared nothing for white shoes. Whatever the explanation for her interest in black, the fact is undoubted. In so far as brightness differences influenced choice, they must have tended to enhance the value of the darker colors; but since black itself ranks considerably lower in the scale than any one of the colors, there is no reason to suppose that brightness was of great importance in determining color preferences.

As a sort of control test for the colors, I conducted during the same period a similar series of experiments with material toward which I thought the child would be indifferent. I chose for this purpose the large sized square and circle of the kindergarten gifts. The method and conditions of the experiment were the same as those of the color tests. Out of 70 choices, 34 were for the square and 36 for the circle; a result which is in marked contrast to the reactions to brightness and color differences, and indicates that mere form was in fact of no interest to the child.

The results obtained in this series of color tests are not in accord with the best accepted opinion up to the present time. Miss Shinn[1] in her excellent summary of the material at hand, comes to the conclusion that color vision probably does not develop until the last quarter of the first year, and that red is the only color perceived until the second year. She finds no evidence of the perception of either blue or green until the latter half of the second year, a time when most children learn color words.

If it were not for the series of tests which I have reported; if I had depended on mere observation, I should certainly be of the same opinion as Miss Shinn, except that I should be doubtful of the perception of even red during the first year and a half. In observing the child on whom these tests were made, I have up to the present time (16½ months) seen no further convincing evidence of color vision. I am confident that the series of experiments I have reported, if performed any time between seven and seventeen months, would have yielded negative, or at least ambiguous, results with regard to the perception of color. My infant has within the last two months displayed the usual interest in learning words, particularly the names of objects in which she is interested. She cannot herself say the words, but she shows her understanding of them plainly by her responses. She also understands a few qualitative words, though no effort to teach them has been made. Thinking that the time was ripe for learning the color names, I have again given her the colored discs used in the tests, and have attempted to teach the word

[1] *Notes on the Development of a Child*, 1907, II., p. 159 ff.

red, but so far without success. The amount of time and effort I have expended on it would be more than sufficient to teach the name of an object in which she was interested. In this point my experience coincides with that of other observers. Miss Shinn reports that all the children of whose vocabularies she has records learned not only other words, but other qualitative words before those for color.

This series of facts, if the facts can be regarded as established, needs further interpretation. Let me summarize. Color vision has been shown to exist in the case of at least one infant at six months. Neither in the case of this infant, nor in that of any other on record is there convincing evidence of color vision in the period between seven and sixteen to eighteen months, except a few cases of interest in red and more doubtfully yellow. Experiments conducted during this period have yielded ambiguous results.[1] The next conclusive evidence of color vision coincides with the period of learning color names, a stage which is reached by most children some time between the sixteenth month and two years. Words for color are always acquired later than some other descriptive adjectives.

There are three problems presented by this series of facts for which I would like to offer a tentative solution: first, why after becoming capable of color vision, the child should give so little evidence of it for so long a time; second, why the child's interest in color vision should display itself just when it does; and third, why other descriptive adjectives should be understood by the child before those for color.

All of these problems seem to me capable of explanation by the laws of interest and attention. In observing the infant on whom these tests were made, I have been very much impressed by the extent to which the young infant displays certain dominant stages of development of interest in his world, which determine not the possibilities of perception, but the actual discriminations made. My first tests were made at a time when I felt sure that the child was absorbed in sensory experiences, or, if the word sensory involves the psychological fallacy, in a

[1] Miss Shinn's criticisms of Professor Baldwin's tests (*loc. cit.*, p. 155) seem to me valid.

largely passive experiencing of the world. She was intent on listening to sounds, looking at objects, and comparing them visually, or alternately looking at and feeling of objects. In all these activities it is of course true that her attitude was not entirely passive. She was making accommodations to her world, and motor coördinations were being developed. But what seemed to be uppermost in her consciousness was not the movements she was making with eyes or hands, but the sense impressions she was receiving. The movements were a mere means for obtaining a clearer sense impression. In my experiments she first compared the two discs visually, and then grasped the one she wanted and brought it nearer her eyes for a more careful look. At about the end of the sixth month, when the act of grasping had become precise and easy, and had begun to develop into the ability to manipulate objects, the center of interest seemed to shift quite rapidly from the passive to the active aspect of experience. It was no longer a question of what sense impressions were being received from an object, but rather of what manipulations could be made with an object. She had discovered her capacity to act and had ceased to be a mere spectator. Only objects that could be handled held the child's attention for any length of time and she became endlessly eager to get hold of new objects. In my experiments, it was as though she ceased to care whether the paper was red or gray, but cared only that it was something she could crumple and tear. The stage of absorption in the process of manipulating objects has lasted unimpaired up to the present time. The child's life has been devoted to such activities as putting the cover on to a box and taking it off again, turning over the leaves of books; putting her toys into a basket and taking them out again, trying to put on her own clothes, and taking off the doll's shoes and stockings. When I give her colored discs to play with, her whole desire is to do something with them; to put them into a box and take them out again, or bring them to me and take them away again, or put her finger and mine through the hole in the disc. For all these purposes one disc is as good as another. When I demand that she shall bring me only the red one, or put only the red one into the box, I am in-

terfering with the normal course of her activities, and she only gets angry and impatient. She does not understand, not because she is incapable of experiencing the colors, but because nothing in her activities at the time hangs upon their discrimination. The child, like the adult, attends to and discriminates only those aspects of experience which are of importance in carrying out his purposes.

But why, to take up my second point, at a somewhat later period does the child suddenly find that color is of importance to him? The child whom I have under observation has not yet reached that point, and in attempting an interpretation of the fact I feel on less certain ground. It seems to me, however, that it marks another shift of attention, conditioned by the acquisition of a degree of mastery over the process of manipulation. It is the general law of attention, that to the extent to which the process which has been absorbing it has become habitual, to that extent does attention shift to some new phase. As soon as one problem is solved, another arises. By the time a child is about eighteen months of age, he has become familiar with the common objects about him, and knows with some accuracy what can be done with them. The mere handling of objects is therefore no longer so absorbing as to occupy the field of attention exclusively. On the other hand it is a sufficiently well coördinated activity to serve as a means to some further end. The child's attention shifts to making further sensory discriminations because it is free to do so, and because he has become capable of a more differentiated response to his world, a response which demands more discrimination.

Miss Shinn's account of the development of her niece's interest in color offers an excellent illustration. She began to exercise herself in color discriminations by pulling the books out of the bookcase, and calling the color of each one as she did so. At an earlier period the mere act of pulling out the books had been in itself an absorbing activity. When it had, through practice, become a well coördinated and partly habitual act, her attention was free to pass on to new aspects of the experience, and she began to discriminate among the books. The act of pulling out books had became sufficiently habitual so that it

could be used as a means of obtaining this book or that, and distinctions among the books became of importance accordingly. Though, as I have said, my own infant has not yet arrived at an interest in color, I can see signs that her attention has begun to shift from the process of manipulation. Until very recently, for instance, all she cared to do with a book was to open and close it, or turn over the leaves. She was impatient of any attempt to interfere with this activity by trying to call her attention to the pictures, though she had shown her ability to interpret pictures under favorable conditions. Now she voluntarily stops to look at the pictures.

The third point which I wished to discuss is the interpretation of the fact that most children learn other descriptive adjectives before those for color, and even the words dark or black before color words. To Miss Shinn the facts point toward a late development of color vision itself. To quote her argument: "Can we consider the wave of attention and discrimination as due only to the new power to *name* the perceptions? — a power that is always intensely interesting to the child, and that leads him often to discriminating observation of things he had scarcely noticed before? It would really beg the question to say so; why should the power to name the perceptions be delayed to this period, when other concepts, which seem to us much more abstract and less obvious, are rapidly coming to expression? . . . And I cannot see that there would have been any more advanced analysis in pointing at a red ribbon, or blot of red ink, and crying 'red,' than in pointing at a blot of black ink, or a coal smutch, and crying 'black.' Yet the *red* identification seemed impossible to my niece for two months after the *black* one was easy; to Mrs. Hall's boy for at least four months. Both these children, moreover, used several other descriptive adjectives before color names appeared; and while no other record corroborates mine as Mrs. Hall's does with regard to the early appearance of *black* (dark, however, often appears early), all the vocabularies in my hands show that other adjectives precede those of color, by a considerable interval — two or three months, even up to eight months, in all cases where I can fix the dates" (*loc. cit.*, p. 163).

It is doubtless true that pointing at a red object and crying 'red' is no more difficult a piece of analysis than pointing at a black one and crying 'black'; and yet there may be a reason why the wave of attention and discrimination should, as a matter of fact, take one direction rather than the other. That is to say, there may be circumstances in the conditions surrounding the average child which lead him to make certain kinds of discrimination earlier than others of apparently equal ease. In fact every child makes an enormous number of sensory discriminations whose temporal order must be determined by some other factor than that of difficulty. Now it seems to me that there are circumstances in the surroundings of the average normal child which should lead him to single out some other qualitative aspects of experience earlier than color. To apply the laws of attention once more, the child attends to those aspects of experience which are of importance in directing his activities. So long as the child's activities are still in the stage of mastering the simple, gross manipulation of the objects about him, color is a factor which is rarely of crucial importance. The quality hot, on the other hand, is sure to be early learned because of its obvious practical import. Hot objects are objects which must not be handled under penalty of immediate and certain pain. Hot food is food which must not be eaten. My own infant, though as I have said, she does not as yet understand color words, knows the meaning of the words 'another,' or 'the other' perfectly, concepts which at first sight seem more abstract than color. Her mastery of them has come about simply enough in the course of her activities. She is very fond of trying to put on her own shoes or my slippers. After she has succeeded with one, I say "Where is the other?" and she understands because the word denotes something that is of importance to her at the moment. Again she is putting her blocks into a basket, and I keep asking for another block to put in. The words now have a generalized meaning for her. But in trying to teach the color words, I have failed to create a situation where the color is of any inherent value in controlling her activities. What she can do with one disc, she can with another. I have tried to throw an artificial stress on the red by

arbitrarily refusing to accept any other to put into the basket, or shut up in the box. In time I think even this arbitrary condition will bring about the desired discrimination, but the child knows as well as I do that my restriction is arbitrary, and that as far as her interests are concerned one disc has no advantage over the others. What I have called out so far is indignation at my arbitrary interference, not discrimination of the red. The child recognizes the spoken word perfectly well, but wishes to apply it to all the discs.

The same kind of difficulty has confronted me in trying to teach the words thumb and finger. She recognizes both words, but wishes to apply them both to all the appendages of the hand. There can be no doubt, of course, that she perceives the thumb as a separate object from the fingers, but there is no situation where it is important to her to single out that appendage and set it over against the others. The words square and circle, too, with as much effort, have had as little success as the color words.

The fact that most children learn the word dark early, and that some of them learn black before the colors seems to me capable of a similar pragmatic interpretation. The experience 'dark' is of obvious practical importance to the young child. A dark room is one in which all his activities are impeded, and, moreover, usually means going to bed. One would have to know the circumstances under which the word black was learned by the children to whom Miss Shinn refers to know whether the same principle applies, but a very probable way for the word to become of early importance is with reference to dirty, black hands which have to be washed, or a black dress which must be changed.

ON OCULAR NYSTAGMUS AND THE LOCALIZATION OF SENSORY DATA DURING DIZZINESS.

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If a person sitting with his eyes closed and his head in an upright position is slowly set in rotation about a vertical axis lying within or near his head, he meets with a considerable variety of sensory experience. All these sensory data contribute more or less toward the perception of motion, but in different ways. And these data fall into three general groups which it takes no extremely subtle introspection to distinguish.

1. Firstly, one distinguishes a group of sensations which proceed from extra-peripheral stimuli: currents of air are felt by their impact or their temperature, on any uncovered surfaces of skin; any source of light which can be dimly perceived through the eyelids or eye-bandages, will be seen intermittently as with each rotation the face is brought opposite the light; any source of sound will be heard alternately loud and faint as with each rotation an ear is twice presented to it. The air currents would of themselves give no clue to the movement of the person's own body, and they are ordinarily felt as somewhat irrelevant data, which hardly even tend to fuse with the other sensations of motion. The visual and the (doubly rapid) auditory intermittences become, with increasing speed of rotation, rhythms of which the spatial or temporal significations are subject to considerable individual differences. Thus for my own introspection the visual intermittence becomes a temporal rhythm, while the auditory sensations become a hoop of sound lying horizontally about the head as a center, and having two spots of maximum loudness opposite each other, and two of minimum loudness midway between them. I have sometimes, though seldom, had from the visual intermittence a comparable hoop of light. In any case the person is aware of a relative motion

between his body and the hoop of sound, or light, but which of these is at rest and which in rotation is so far ambiguous and is determined by the factors given in group 3. Both the visual and the auditory phenomena are readily isolated in introspection, and both are felt to be distinctly 'secondary criteria' of motion.

The three sorts of sensation so far mentioned may all be eliminated by fairly simple precautions (though they are also readily ignored by the observer), but another kind of sensation is neither so readily elimated, nor ignored, nor distinguished from the sensations of group 2. This kind comprises the tactual sensations mainly of the hands, back, thighs and soles of the feet, which vary with the inertia of the body and with the centrifugal moment induced by the rotation. These sensations, while less clearly a secondary criterion of motion, while more intimately associated, that is, with the sensations of group 2 and even of group 3, can still after some practice be distinguished as tactual sensations of varying strength.

2. Secondly, there are the sensations from proprio-ceptive organs (Sherrington, '06, p. 130) in joints, muscles and other tissues, which are stimulated (similarly to the last-named class of group 1) by the inertia of the trunk, limbs, internal organs, and even perhaps of the blood, and by their centrifugal moment. Sensations supposed to be stimulated in this way in the cerebellar mass or its sensitive coatings were originally adduced by Purkinje (Aubert, '88, S. 119–120) to explain dizziness; and in connection with movements of translation Delage ('86, p. 623) has referred to sensations seemingly "produced by a sort of internal tidal movement in which all the liquids and such solid organs as have any mobility, participate." While such factors are hypothetical, certain sensations from proprio-ceptive organs in joints, tendons and muscles undoubtedly play a part in the perception of the motion of one's own body (Schäfer, '87; Breuer, '90, S. 204; Mach, '73, S. 127; Abels, '06, S. 382). It is difficult even after practice to distinguish introspectively these sensations from those of group 3, except when, with a high speed of rotation, they become intense, whereupon they are readily distinguishable as secondary criteria, from the true

sensations of movement of group 3. How much these proprioceptive stimulations when not intense contribute to the perception of movement is hard to determine. Some writers, as Abels ('06 and '07), have wished to find in them the very basis of that perception, but this is an unwarrantable view for as Mach ('74, S. 130) has said: "One can scarcely explain feelings of motion in terms of skin or muscle sensations, in view of the feelings in the head, the enormous influence of the head position, and Flourens's experiment:" and indeed Mach might have added, in view of well-nigh every fact that experiment has yielded regarding the canals and sacs of the ear (cf. also Breuer, '07). Or, as (Crum) Brown has said ('95, p. 15), "A few experiments . . . will convince any one that we have here to do with a perfectly definite sense, and not with any vague sensations caused by the inertia of the soft parts of the body." Nevertheless the proprio-ceptive sensations are of interest here, and deserve more experimental notice than they have so far had.

3. Thirdly, there are the true sensations of motion which are in some way dependent on the semicircular canals, and probably the sacs, of the ear. These present a remarkable complication of phenomena, with which we shall have chiefly to deal in the present paper. And firstly introspectively. If a person sitting with his eyes closed and head upright is rotated about a vertical axis within or near his head, and if the speed of rotation increases continuously, the person feels his body to be rotating in the direction of the actual motion, and he also generally feels objects in the space around him (by as much as he is aware of them) to be moving more or less rapidly in the opposite direction. Two things, in short, the body and the objects around it, are felt to be in relative motion.

Problem I. — What organs yield the sensation of rotation?

I believe that it has not so far been noticed that unless the rotation is very rapid, the *direction of the attention* is able to determine which of these shall be felt to be the more involved in motion and which to be almost or quite at rest. If, namely, the person 'directs his attention' to his own person, *this* will seem to be in rapid motion while then the environment may

seem to be quite at rest: but if the attention is directed to the environment sweeping by, if, that is, the sensations of groups 1 and 2 occupy the focus of attention, the objects about the person will seem to whirl rapidly to the rear while his own body will seem to be nearly or quite at rest. I find also in this case a faint suggestion in consciousness of a space far behind these dimly presented objects, which is, like my body, at rest. When the rotation is rapid, however, it is much more difficult and often impossible to achieve such a 'setting' of the attention. Also, as we shall later see, some individual differences are to be expected in this field. This influence of the direction of the attention on the perception of motion during rotation is doubtless analogous with the effect mentioned by Hering of the same factor, on the apparent position of objects.

Problem II. — What is it which is involved in what we introspectively call 'setting the attention,' which in the case of rotation can shift the appearance of motion from one object to another?

If the rotation is long protracted at an ever-increasing rate of speed, the person becomes sick. This phase is no part of dizziness proper, and does not here concern us. The remaining introspective phenomena which interest us occur when the speed of rotation is decreasing, and after the rotation has stopped. Now when the acceleration of motion changes from positive, or zero, to negative, the person feels without appreciable latency *both* himself and the objects around him to be rotating in the contrary direction. Here, too, the direction of the attention is of influence, but here the attention is to be directed *against* the illusory motion, stemming the tide as it were, and then the movement *both* of the body and of such sensations as one has of the outer objects (group 1) is alike diminished or annulled.

Problem III. — How does setting the attention against the illusory post-rotary movement reduce the apparent motion of both the body and the enviroment?

If now the eyes are opened, the apparent rotation persists (although cf. Bárány, '06, S. 223) save that the visual field is far more prominent than before; and it is still whirling con-

trariwise to the original rotation. As Mach ('73, S. 127) has described it: "As soon as the apparatus is slowed down one has the feeling of making a contrary rotation together with the box [in which one is enclosed]. If now the box is opened, entire visual space with its contents rotates. *It is as if all visible space were turning within a second space which one believes to be motionless, although it is identified by no visible cue.* One might almost believe that there exists behind visual space another space to which the visual is always referred."

Furthermore, during the decrease or immediately after the end of the rotation, objects presented to the tactual sense are felt to be in contrary rotation, similar to that of the visual field. This tactual dizziness is far less pronounced than the visual, but is sufficiently attested by Purkinje ('20), Mach ('00, S. 100), Wundt ('02, Bd. 2, S. 586) and others. There is likewise an auditory dizziness quite analogous to the preceding, whereby sources of (continuous) sound appear, after the rotation, to rotate contrariwise. As Münsterberg and Pierce ('94, p. 475) have described it: "If after the rotation, but while the eyes were still closed, the sound was given continuously for a time, it seemed to make the illusory movement too: it remained, that is, in constant orientation with the body." These visual and auditory phenomena experienced *after* rotation are not, of course, to be confused with the 'hoops' and other phenomena of group 1 experienced *during* rotation.

The foregoing phenomena immediately suggest the following problems:

Problem IV. — Why does the body appear to reverse its motion and to rotate contrariwise when the acceleration becomes negative and after the actual rotation has ceased?

Problem V. — Why after the rotation has ceased does the visual field continue to rotate contrariwise?

Problem VI. — Why do tactual and auditory impressions likewise continue to rotate contrariwise?

While the behavoir of the visual field has since the early seventies been referred in a general way to the ocular nystagmus which is normally induced by rotation, the connection between the two is still susceptible of elucidation: and I am not aware

that any specific explanation has been offered for the tactual and auditory dizziness. It was with these problems in mind that I undertook in the fall of 1908 some experiments, on which the following discussion is partly based. It is also based in part on some experiments with dizziness previously described by me ('06), and on the literature of the subject.

I. What Organs Yield the Sensation of Rotation?

On this question a very large number of investigators have come, although in a very general way, to some appearance of agreement. In general nearly all assent to the hydrokinetic theory of Mach, Breuer and (Crum) Brown,[1] and I believe that in the main this theory is established beyond all question while some of its details will bear further examination. The experimental facts show that the semicircular canals of the ear are stimulated by circular and rotary motions, and that the utricle and saccule are (almost certainly) stimulated by motion of translation. (Some writers dispute the latter point, although, as it seems to me, on hardly sufficient grounds.) And from this the conclusion is commonly drawn, that these organs yield *directly* the sensations of rotation and of translation. The argument is —(1) sensations of motion depend on the position of the head; (2) the only receptor organs situated in the head, which are stimulated by motion, are the ampullæ and sacs of the ear; (3) therefore the sensations of motion are sensations from the ampullæ and sacs. Now clearly the two premises warrant the conclusion that — therefore the sensations of motion *result* from stimulation of the ampullæ and sacs. But these sensations need not result *directly*, as the first conclusion affirms. Nor have I so far discovered any experiments adduced explicitly to show that sensations of motion result directly rather than indirectly, from labyrinthine stimulation, although the former is commonly assumed. Bárány ('06, S. 265, S. 275–6) touches on this point and declares: "It is an error to say that excitations thereof [*i. e.*, of the canals and sacs] do not come to consciousness; *strong excitations of them do come to consciousness*, either as

[1] This theory has been admirably summarized by Nagel ('05, S. 790) and by Peters ('05).

such or in combination with [unter Mithilfe d.] the accompanying eye-movements" (eye-movement sensations?). But the ground of this affirmation is neither here nor elsewhere made clear. The only other reference to just this point, which I have found, is in Ewald ('92, S. 133): "There are not merely special physiological movements, which take place during and after rotation, but also these are accompanied by special sensations. But the relation existing between the two has never been made clear. It has been taken for granted that the abnormal movement ensues on an abnormal sensation, and is in a way its visible expression. But I do not believe that the relation is such a simple one." And in another place (S. 141) Ewald finds the movements following rotation to be 'reflex,' by which he clearly means that they are produced directly by the labyrinthine excitations *without* these latter having come to consciousness. There is, then, ground for debate in this matter.

On the other hand it has been generally granted that the nystagmic movements of the eyes are closely connected with the visual dizziness. Thus at the very outset Purkinje attributed visual dizziness to 'unconscious' eye-movements carried over [übertragen] to outer objects (Aubert, '88, S. 117). Delage, too, while deeming the labyrinth an organ of sensation in the strict sense, ascribes features of visual dizziness to movements of the eyes ('86, p. 610–11). Mach ('00, S. 98–100), Breuer ('98, S. 499) and Kreidl (*ibidem*) also attribute visual dizziness and illusions as to the vertical, to nystagmic and compensatory eye-movements; although these writers too believe in direct labyrinthine sensations. The accepted view should seem to be, then, although I do not know of an explicit statement to this effect, that movements of one's own body in rotation and translation are perceived by means of sensations coming directly from the ampullæ and sacs, while visual illusions of rotation and many of motion and position are due to reflex (and 'unconscious') eye-movements. Motion of translation of one's own body would be perceived by means of sensations from the sacs, of rotation by sensations from the semicircular canals.

Now there stands in somewhat surprising contrast to this view the experimental fact that both during and after rotation

the sensation of rotation of one's own body is instantly inhibited if the ocular nystagmus is inhibited. This observation was first made by Bárány ('06, S. 224): "*The direction of one's line of regard is also of influence on the illusory sensation of rotation. If I have nystagmus horizontalis to the right* [*i. e.*, the rapid eye jerk toward the right and slow movement toward the left], *which I can inhibit by looking toward the left, and if with my eyes closed I do look toward the left, the sensation of apparent rotation stops at once — just as the apparent motion of outer objects had* [previously and for the same reason] *stopped. If I look again to the right, the illusory motion commences again. One can observe several such disappearances and reappearances of the sensation of rotation. A considerable number of physicians was able to observe this phenomenon, which I am the first to describe, and I have had the same reports from enquiries among patients.*"

I have previously reported ('06, p. 72) that the slow phase of the ocular nystagmus can "not voluntarily be inhibited; whereas the swift movement is so far voluntary that it can be *inhibited* at pleasure. It is possible, that is, to fix the eyes on that side of the field toward which the slow movements are directed, but not on any point at the other side of the field." And this inhibition of the nystagmus always inhibited visual dizziness; but I had not at that time noticed that it also inhibits the apparent rotation of one's own body. Now conflicting statements are to be found on this point, and the most emphatic are those of Mach, who states ('74, S. 123) "that a person can have very marked subjective phenomena of rotation with demonstrably fast fixation and no eye-movements. If on the inside of the paper box described in the previous communication [the observer was inside the box, and both rotated] there is fastened a black cross on a white ground, so that when one fixates the crossing every deviation of the line of regard is betrayed by an after-image, then one observes no such after-image when dizziness starts up. One can fixate and still feel dizzy. I have also convinced myself by direct observation of the eyes of a second observer, that the eyes can remain at rest when the experiment is carried out in the way I have described."

And twenty-six years later Mach again wrote ('00, S. 101–2) to the same effect, although here it is *visual* dizziness rather than the sensation of bodily rotation which is not inhibited by voluntary fixation of the eyes. For Mach both kinds of dizziness undoubtedly persist.

It chanced that a few weeks before learning about the observations of Bárány, two other observers, Dr. Tait and Mr. Ricker, and myself noticed (accidentally, for we were then interested in visual dizziness) that voluntarily inhibiting the nystagmus does away with the sense of bodily rotation not merely after the rotation has stopped, but *during the actual rotation itself.* We were all three able repeatedly to undergo a lively passive rotation (axis of rotation vertical, head erect and over axis, eyes closed) of one to two minutes without *at any time* having the sensation of bodily rotation.

Such an inhibition of nystagmus *throughout* the experiment can be accomplished only in this way: Before the chair is set in motion (by a second person) the observer directs his fixation as far as possible in the direction *contrary* to the coming rotation, and holds his eyes in this position as long as the acceleration remains positive. With the eyes closed, as here, this requires some practice and we found that it could be facilitated by securing a fairly durable after-image on the retinæ immediately before the experiment. When the acceleration has nearly reached zero, *i. e.*, when the speed has become nearly constant the observer relaxes his fixation and lets his eyes do as they will. They wander slowly toward the primary position of regard and remain there as long as the acceleration stays at zero. No motion of the body is felt if the voluntary control of the eyes is relinquished at the right moment. As soon, now, as the motion begins to be reduced (the acceleration is negative) the eyes wander involuntarily to the other side, *i. e.*, *with* the actual rotation, and here they must voluntarily again be fixed until several seconds after the rotation has actually ceased. The experiment requires that the observer shall not actively assist to rotate himself.

If this is successfully accomplished all sensations belonging to group 3 are inhibited leaving, however, those of groups 1

and 2. The 'hoops' (group 1) continue to rotate contrariwise as long as the actual movement lasts, but no longer (although they would continue contrariwise still longer if the nystagmus were not inhibited), and the centrifugal sensations are distinct in consciousness; and yet so insignificant are these secondary criteria of motion as compared with the primary sensations thereof, that the subject feels himself to be at rest in a somewhat remote though whirling entourage. This motion of surrounding objects is far from being adequately realized, as we ascertained occasionally by opening the eyes during rotation, whereupon the sudden realization of the rapid movement (backward and contrary to the actual movement) of visible objects came as a shock. But even this does not reinstate the sense of one's body being in motion provided that the nystagmus is still inhibited. It startles one sometimes into relaxing the hold on one's chair, so that we found it to be very disagreeable and somewhat risky to open the eyes while the nystagmus (and therewith the feeling of rotation) were being suppressed *and* the rate of rotation was rapid. The three observers above mentioned were well trained in the observation of dizziness; another subject, with less training and rather easily nauseated by dizziness, underwent the rotation without a sense of being himself in motion: and two women, quite untrained in the matter of dizziness, suppressed the post-rotary feelings of bodily rotation on the first trial, by inhibiting the post-rotary nystagmus; and on second trial succeeded in feeling no motion during as well as after the rotation. In all the experiments I observed nothing which would lead to any other conclusion than that voluntary inhibition of the ocular nystagmus *directly* inhibits the sensation of the rotation of one's own body.

These experiments go wholly to confirm the observation of Bárány which was given above. And we must now consider the precisely contradictory testimony of Mach. Since there is not the slightest ambiguity in the form of his statements, there remain three conceivable ways of reconciling them with the other observations above reported. It is possible that with Mach and his subjects the nystagmus was not really inhibited, for not all who try to inhibit it succeed. This is rendered

plausible by the fact that Mach nowhere speaks of inhibiting the nystagmus by fixating toward the *side* contrary to motion, and in the absence of such a statement one must suppose that the inhibition of nystagmus was attempted by trying to hold the eyes voluntarily in the primary position, *i. e.*, straight forward (cf. the above quotation from Mach). Now I am personally quite unable to inhibit the nystagmus, either during or after rotation, in this way, nor have I seen another person who was able to do this; and it is clear from Bárány's observations ('06, S. 215–17) that such an attempted fixation straight ahead might actually augment rather than decrease the nystagmus. Nor can I attach much importance to the after-image test above quoted, with the black cross on a white ground, since there is no vision during the quick phase of the nystagmus, as we shall see later, and since the slow phase is too slow to leave a perceptible after-image streak unless the stimulus (here the 'white ground') is very intense.

Yet aside from this Mach says that he examined the eyes of another observer during the voluntary inhibition. And while this is very difficult, since Mach himself must also rotate, while also nystagmic movements sufficient to produce dizziness can be so minute as to need a reading telescope for their discovery (Bárány, '06, S. 214), I cannot think it probable that Mach would have convinced himself, as he says, that this observer had inhibited the nystagmus if such had not really been the case.

A second possibility would be that Mach and his subjects mistook centrifugal sensations (group 2) for the true movement sensations of group 3. (Sensations of group 1 give no feeling of *bodily* motion so long as the nystagmus is inhibited.) This would be very probable with observers of little experience, but it can hardly have happened with Mach; and furthermore he says that visual dizziness also continues after the inhibition of nystagmus, and centrifugal sensations could scarcely have been mistaken for visual dizziness.

The third alternative remains, that we have here a true case of individual difference. And one must be the more willing to admit this here since it is not more remarkable than other mani-

fest discrepancies among the observations of careful experimenters in this same field. Thus Bárány, for instance, gets post-rotary dizziness of his body so long as his eyes are closed, but this is *supplanted* by visual dizziness when he opens his eyes: whereas Mach, and most other observers, feel with the eyes open both kinds of post-rotary dizziness *at once* (Bárány, '06, S. 223). Or again, in post-rotary visual dizziness Bárány sees the visual field oscillate from side to side in both directions ('06, S. 221); whereas Mach, Breuer, Delage and most other observers see it whirl contrariwise to the preceding actual rotation (Nagel, '05; Peters, '05); and Helmholtz ('67, S. 603; '96, S. 747) saw it whirl sometimes with and sometimes contrary to, the direction of the preceding rotation. Still more extraordinary are the different observations as to the localization of a visual after-image with the eyes closed, during voluntary and involuntary eye-movements. Indeed there are few branches of psychology where entirely credible observers more widely disagree regarding simple matters of fact. And I should designate this branch as the one comprising the following four things and their interrelations — motion, muscular contraction, the voluntary innervation to contraction, and the perception of movement.

Granted, then, the fact of unusual individual or typical differences, it remains to study the several types in and for themselves, in the anticipation that in the end some explanation will be found which will reconcile all discrepancies. I have not so far seen a subject who, like Mach, experiences bodily and visual dizziness after he has inhibited his ocular nystagmus, but I shall look for such persons, and meanwhile return to the discussion of such cases as Bárány and I have met. For some things are inevitably true of these subjects, whatsoever else may be true of the members of other types. Now we have seen that for the subjects who are at present in question, voluntary inhibition of the nystagmus inhibits the sensation of bodily rotation. Bárány ('06, S. 275–6) has sought to interpret this fact: "We have further seen that inhibition of the nystagmus eliminates the sensation of rotation. Since the voluntary direction of the regard can scarcely effect an inhibition of impulses coming over

the vestibular nerve, we seem bound to conclude that the nystagmus as such, the involuntary and unconscious movement of the eyes, is of influence on the production of the rotary sensation; perhaps, indeed, that it is the nystagmus center in which the above-mentioned integration [Verarbeitung] of the vestibular impulses and the excitations occasioned by changes of the head position, takes place. I perhaps went too far in an earlier paper, where I said that inhibition of the rotary sensation through inhibition of the nystagmus, proves that the nystagmus and not the vestibular impulses cause the rotary sensation. *It is sufficient to assume that for the production of rotary sensation such impulses are necessary as, owing to the nystagmus, are delivered to the center involved, and that these form so large a component in the integration of the subcortical impulses* [*?*] *that the inhibition of them suffices to prevent the sensation of rotation from being produced. Nothing but a case of total, bilateral, oculomotor paralysis of central origin could really decide the point: in such a case there ought to be no sensation of rotation.*" I quote this passage in full (with italics as in the original) because it bears so explicity on our theme. I understand Bárány's conception to be that afferent vestibular impulses and afferent impulses from eye-movements are combined in a subcortical center, from which they emerge in consciousness *as* the sensation of rotation: and that the latter components (which would ordinarily be called 'sensations of eye-movement') at least are indispensable to the production of rotary sensations. Presumably he would hold the vestibular impulses to be indispensable as well.

But there are alternative possibilities. Is it true, as he declares, that "the voluntary direction of the regard can scarcely effect an inhibition of impulses coming over the vestibular nerve"? The voluntary direction of the regard certainly inhibits whatever impulses those are which produce the *rapid* phase of the nystagmic movement, and I see nothing to warrant a statement on one side or the other as to the relation between the vestibular and the voluntary impulses. It *might* be that the vestibular sensations are the sensations of rotation, but that these are inhibited when the rapid eye jerks are inhibited. And yet on the one hand professed ignorance is better than so far-

fetched and mysterious an assumption as this latter; while on the other hand, regarding the former assumption, we must remember the many cases in which one's body is felt to move, with sensations distinctly like those of group 3, in which neither the semicircular canals nor the sacs can be supposed to be stimulated. A person who stands on a bridge and watches the water flow beneath, from time to time feels himself moving contrary to the flow of the water (Mach, '00, S. 104). Helmholtz ('96, S. 763–4) mentions that when the dome of an astronomical observatory is turned about, a person standing beneath it is apt to feel the floor and himself rotating contrariwise. And there are many other such illusions of bodily translation or rotation, not distinguishable from the sensations of group 3, in all of which the stimulus is purely visual and *there are no* afferent vestibular impulses. These considerations, I believe, quite shut out the vestibular impulses from being essential to the sensation of motion of one's own body.

We have next to examine Bárány's second and indispensable component — the afferent impulses occasioned by the nystagmic movements. Before we can suppose such impulses to be essential, or even in any wise contributory to the perception of motion, we must answer satisfactorily the arguments so cogently, and one might almost say savagely, stated by Hering ('61, S. 30–32): They are, he says, "proof enough that only the displacement of retinal images, and not sensations of tensions in the muscles, acquaint me with changes in the position of my eyes." In short, Hering allows no share at all to eye-muscle sensations in the perception of eye-movement; and he is disinclined to allow even their existence. Is it, then, conceivable that they afford sensations of movements of the whole body? We must also bear in mind that the famous discussion between Plateau, Oppel, Helmholtz, Dvořák and others did not confirm the belief in the existence of eye-muscle sensations. We have also had recently, from Dodge ('04 and '07), Judd ('05) and other investigators in the Yale Laboratory, fresh evidence that sensations of eye-movement play little or no part in the perception of space. Personally I am unable at present to dispense with 'eye-movement sensations' as a part of my psychological

furniture, and yet in the present case I must admit that several facts seem to exclude them from assuming any importance. It is, apparently, the efferent motor impulses to the eyes rather than afferent impulses yielded by eye-movements that have taken place, that most closely parallel, or as some persons might say, *are*, the sensations of bodily movement.

These facts are, firstly, that of the two phases exhibited by the nystagmus, the rapid and the slow, one phase but not the other seems to cause sensations of bodily movement. This is the rapid phase. Under positive acceleration the rapid eye-jerk is *with* the rotation, and in this same direction the body is *felt* to turn; and as soon as the acceleration becomes negative the rapid jerk takes place contrary to rotation, and the body is felt to reverse its motion although it is actually rotating in the same direction. At high but *uniform* speed, of course, there is no nystagmus and likewise no sensation of one's body being in motion. As Bárány has said ('06, S. 225), "*the apparent rotation of one's own body is always in the same direction as the rapid nystagmic movement.*" Now on the sensory side there is nothing, so far as we know at present, to distinguish these movements so sharply from each other; for of course the circumstance that one is fast and the other slow would not account for one of them being 'sensed' and the other not. But on the efferent side there is a prime distinction — *the motor impulses to the rapid movement can be voluntarily inhibited*, while those to the slow cannot be checked (Holt, '06, p. 72). The voluntary attempt to inhibit them, which has to be made by trying to fix the regard in a direction wholly or partly opposed to the direction of the slow movement, results only in *increasing* the nystagmus (Bárány, '06, S. 215–17). (This is why Mach is trying to fixate a point straight in front, is unusually fortunate if he succeeded in inhibiting the nystagmus.) Now certainly the rapid movement cannot be called a voluntary movement, since the whole nystagmus arises involuntarily; but since the rapid phase is amenable to voluntary inhibition, it may properly be called semi-voluntary. Now this circumstance that the rapid phase which alone counts toward the sensation of bodily movement is more nearly related to voluntary effort than the other

phase, is directly in line with those facts already referred to, which Hering so emphasized ('61, S. 30). "A position of the eyes which I have not voluntarily induced, which therefore I did not already know pretty exactly before it took place, and even more a movement which takes place without my special intention — I am totally unable to estimate. . . . [S. 31] If I have voluntarily brought about a position of the eyes, then of course I know beforehand the direction and approximate extent of movement [involved], since otherwise I should not have been able to will just this movement; and both the direction and approximately the amount of force which is necessary for a given movement, are decided [bestimmt] by the will." This is coming, perhaps, very close to 'innervation feelings.' The empirical data to which Hering refers, may be summarized as the general lack of relation between the position of the eyes and the subjective localization of optical impressions. The eye-movement sensations, supposing them to exist, in many cases so inadequately register the eye-movements that a false localization is assigned to the objective sources of visual data. An example will sufficiently illustrate this general phenomenon.

In a previous paper ('06, p. 72) I observed, "it is well known that after-images move with every involuntary eye-movement." Here I relied chiefly on the introspection during dizziness (with eyelids closed) of myself and four other subjects (*ibid.*, p. 70–1), on some observations of my own on the vision of after-images during 'pursuit movements' (Dodge, '03), and I *think* on some printed statements which, however, I can no longer identify. My own observations were, and on retrial still are, unequivocal. But I have since discovered that other observers equally 'well know' that for them after-images do *not* move with involuntary eye-movements: on this point both Hering ('61, S. 30–31) and Bárány ('06, S. 221–2) are perfectly explicit, and several other authors imply the same view. On the other hand Hering's account distinctly implies that after-images *do* move with *voluntary* eye-movements even when the eyelids are closed. Bárány ('06, S. 222) gives this as Hering's view and confirms it himself (S. 221), Exner ('90, S. 50) and A. Nagel ('71, S. 256) affirm the very same. Whereas

Purkinje (Aubert, '88, S. 118) recounts and apparently confirms an experiment of Charles Bell's in which "a blindingly bright image left on the eye after gazing on a shining object, always appears at rest during eye-movements executed in total darkness, and starts into motion only when the eyes are open and the [after-]image can be compared with external objects which are at rest." Now the observations in question are singularly easy to make, even for a novice, and I believe that such extraordinary discrepancies again rest on true differences between observers. (A colleague of long experience in the study of vision tells me that he gets both of the last-mentioned phenomena, but more commonly the latter of these.) Now these discrepancies, if we accept them, prove that there is no direct relation between eye-movements and the localization of after-images, and such a thing is scarcely possible if visual data in general are localized by means of eye-movement sensations. But the main differentia, of which we know, between the above cases lies in the manner of *innervation* — whether this is voluntary, semi-voluntary, or involuntary: the first alone affecting the localization of after-images for Hering and Bárány. And with this view Mach ('00, S. 93–105) also concurs. It is true that this does not remove all disagreement, for I find on myself and four subjects, that localization of after-images shifts with both phases (semi-voluntary and voluntary) of labyrinthine nystagmus, while Hering and Bárány deny this; and Purkinje and Bell find that even voluntary innervation does not affect the localization of after-images. Yet it is, to my mind, simpler to suppose that these divergencies rest on peculiarities of innervation than of eye-movement sensation. And in this Bárány, even more emphatically Hering, and also Mach would agree.

One can of course assume the inhibition, under given circumstances, of the supposed eye-movement sensations and hence their failure to govern localization; but there is nothing to inhibit them in our examples above save the several modes of innervation: so that again the explanation of the phenomenon would lie in peculiarities of innervation. Furthermore the observation of Mach, already described, that the feeling of both bodily and visual dizziness (rotary localization) *can* persist when

there are no nystagmic movements could not be explained in terms of sensation of these non-occurring movements. And if recourse is then had to reproduced sensations of eye-movement, it again appears that only the labyrinthine or voluntary innervations could be effecting such reproduction. The fact that Lotze, Münsterberg and others have declared 'innervation feelings' *to be* reproduced sensations of movement does not affect the present case for the issue here is between afferent and efferent process,— whether incoming or outgoing impulses are more nearly parallel to feelings of motion. To resort, then, to *re*produced kinæsthetic sensations is to yield the point at once and to grant that it is outgoing impulses and not impulses coming in from the eye-movements that govern the consciousness of movement. And while in Mach's experiment his feeling of bodily and visual dizziness cannot have come from sensations of eye-movement, since he says that the eyes were not moving, it may well have come from innervations to eye-movement, innervations which became inhibited at some level lower than their point of origin. It is worth noting that this view, and so far as I know no other view, accounts for the familiar pathological cases in which the innervation to contract a muscle which is paralyzed, produces the feeling of *the intended* movement although the muscle is not actually contracted by the innervation.

If now visual localization is not explicable in terms of eye-movement sensations, the localization of one's body is of course even less so. And thus this latter depends neither on afferent impulses from the eye-balls nor, as we saw before, on afferent impulses from the labyrinth; and yet the voluntary inhibition of eye nystagmus *inhibits* the feeling of movement (changing localization) of one's body. Only one conclusion remains — the voluntary innervation to inhibit the nystagmus, which is really directed as we have seen to inhibiting the *rapid phase* of the nystagmus, suppresses the feeling of bodily motion by inhibiting the (semi-voluntary) *innervation* of that rapid phase. And it will be recalled that the feeling of motion of one's own body is always in the same direction as the rapid phase. Hence it is the semi-voluntary innervation to the rapid nystagmic phase

which is the process most closely parallel to the feeling of bodily rotation. Our first problem was: "*What organs yield the sensation of rotation?*" *And the answer would be that this is not a sensation in the ordinary meaning of that word, but that the process most nearly parallel to the feeling of rotation is one kind of innervation process.* And I believe that this proposition applies to more than rotation, applies at least to all feelings of motion of one's body that are supposed to be given by the semicircular canals.

This result is nearly in line with a view long supported by Mach ('00, S. 95), who says: "The will to execute movements of regard [Blickbewegungen] or the innervation (?) to these, is the very sensation of space itself." The question-mark after the word 'innervation' is presumably out of deference to those who oppose 'innervation feelings.' I should not care to say innervations *are* sensations of space nor, for reasons too general to be discussed here, that innervations *are* the feelings of bodily movement. The phrasing as italicized above is, I think, a somewhat securer statement. But it is clear that our argument, based in several places on observations at variance with those of Mach, comes out to a position not so far removed from his.

It is true that I have not explained all the conflicting observations given above, nor have I, by-the-way, begun to exhaust the anomalies that stand on record in this field. But I believe that the cases which we have considered, if they are facts and not errors of observation on the part of one person or another, necessitate the conclusion to which we have arrived. And whatsoever conclusion other facts lead to, it will not be contradictory to this of ours. We have found empirical evidence of three grades of innervation — voluntary, semi-voluntary, and involuntary — and that these exert a different influence over the inhibition of the feeling of movement. It is therefore probable that different grades of innervation are of different value in producing the feeling of movement. The three grades of innervation doubtless emerge at higher and lower neural levels; but the neural levels are many and hence the grades of innervation may be many. On this, I think, we may base a reasonable

hope of explaining the so-far complicated and seemingly conflicting mass of observations.

The further problems raised in the first part of this paper must be discussed in a sequel, and it here remains to say only this: The conclusion that *innervations* of one kind or another are the process most nearly parallel to feelings of bodily motion, does not, of course, imply that such efferent impulses are created from nothing, as say on the 'psychic plane.' The nervous energy that constitutes these innervations is released by impulses coming more or less immediately from the periphery. For the physiological unit of the nervous system is the reflex arc. And the issue raised by our empirical data between afferent (from the eye-muscles or orbit) and efferent (to the same) is, I apprehend, more precisely stated as follows. Is the nervous process which runs parallel to the consciousness of the rotation or translation of one's body, one in which the afferent or sensory impulses come wholly or mainly from eye-muscles (or orbit), and diverge in the central nervous system, passing out as diffuse innervations to various and so far unidentified members; or is the process one in which the afferent impulses come from various and so far unidentified members, and converge in the central nervous system, passing out as a unified and definite innervation to eye-movement? The latter alternative is the conclusion that we have reached.

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MENTAL DIAGNOSIS BY THE ASSOCIATION REACTION METHOD.

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The object of this series of experiments was to verify the validity of the association reaction method in a number of dissimilar experiments conducted with normal subjects. The first two experiments were so planned that they could be carried out before a psychology class within an hour's time. The entire series was so arranged that we gradually restricted any advantage that the operator had and gave the subjects greater opportunity to conceal their relations to the experiments. This was most successfully achieved in the third experiment which we describe. A further object of the first two experiments was to learn what mental diagnosis a class would be led to make from observations during the progress of the experiment, the point of view being to estimate the feasibility of using this method in the presence of a jury. We also wished to discover, if possible, whether knowledge on the part of the subject of the methods used would invalidate the results, as has been asserted.

The instruments used were a chronoscope, measuring time in one-hundredths of a second, a lip-key used by the operator to start a pendulum on the chronoscope at the time of giving the stimulus word, and a mouthpiece into which the subject spoke the associated words, thereby releasing a circuit-breaker, which records on the chronoscope the time elapsing between the giving of the stimulus word and the reaction word.

EXPERIMENT I.

Mr. H. and Mr. O. acted as subjects, Mr. H. being a junior and Mr. O. a graduate student. In one corner of a dark-room under a gas-jet stood a child's desk on which were placed Joseph Jastrow's book *The Subconscious*, a bottle of red ink, pen and paper, and a child's Christmas story book. A hammer was tied to the gas fixture. On a table in another

corner of the room were an old dusty crushed derby, and a few other things the nature of which will appear in the progress of the experiment. Mr. S. assisted by handing the instructions to Mr. H. and Mr. O. and seeing that they were fulfilled. One of the men was not to enter the room, the other was to follow out the typewritten directions, which were as follows:

"1. Sit down at the desk. Observe that it is a child's desk. Take up the child's book which is at your right on the desk and read it. Is the poem familiar to you? Are you able to get a mental image of Santa Claus? (See front cover.)"

"2. Pen, paper and ink are on the desk in front of you. Write the first page of 'The Night Before Christmas,' using the material at hand."

"3. Pick up the book at your left and take note of the following: (1) Its author. (2) The title. Write the name of the author and the title three times with the red ink."

"4. Untie the hammer which is tied to the gas fixture, and knock on the desk with it three times moderately hard. Then await further orders."[1]

"5. The building in which you now are is fifty years old. Owing to its age and the fact that it is constructed entirely of wood, there is the greatest danger of fire at any time. In fact the danger is considered so great that the rooms on this fourth story have been abandoned. The building and its contents are heavily insured. The university does not believe it wise to assume any risk. A fire might break out just now. There is a carpenter shop in the basement with wood and oil. What would you do in event of fire? Spend the next few minutes until the clock strikes in devising a plan of escape, if access to the stairs were shut off because of fire. When the bell strikes turn to No. 6."

"6. Back of you there is a door partly open and near you there is a rope. This rope is long enough to reach the fifty feet to the ground. Take up this rope and follow it hand over hand for fifty feet from the door,[2] then come back the same way and leave the room."

[1] At this juncture the assistant came into the room with an interval clock set to ring in seven minutes and told him to proceed with No. 5.

[2] This rope led fifty feet back into a dark attic.

"Please do not talk with anybody about the room or what you did in it."

These instructions having been carried out, the assistant brought one of the men into the lecture room before the class and we proceeded to take the associated reactions, the results being as follows:

TABLE I.

RESULTS OF EXPERIMENT I.

No. of Word.	Stimulus Word.	Results for Mr. H—.		Results for Mr. O—.	
		Association Word.	Association Time.	Association Word.	Association Time.
1	Tree.	Tree.	1.36		
2	Wood.	Log.	1.50	Tree.	.87
3	Sun.	Boy.	1.30		
4	Sky.	Air.	1.82	Story.	
5	Father.			Mother.	1.80
6	To speak.			Radiator.	3.25
7	Grass.	Lawn.	.90	Brown.	.90
8	Sweet.	Sugar.	.97	Bitter.	1.30
9	Ten.	Twenty.	1.45	Men.	1.67
10	Blue.	Sky.	1.20	Seat.	1.70
11	Chair.	Desk.	1.37		
12	Girl.	Boy.	1.10	Boy.	1.60
13	Supply.	Breakfast.	1.47	Tonight.	2.40
14	To fly.	Bird.	1.07	Bird.	1.17
15*	Christmas.	New Year.	1.70	Snow.	1.67
16*	Interval.	Minute.	1.67	One Minute.	3.25
17*	Skull.	Skeleton.	1.52	Neandertal.	3.70
18*	Sleepy.	Man.	1.00	Boy.	2.40
19*	Joseph.	Boy.	1.00	Egypt.	3.15
20*	Ink.	Black.	1.40	Black.	1.05
21*	Creature.	Man.	1.80	Mouse.	1.70
22*	Black.	Ink.	.85	Night.	2.85
23*	Hammer.	Anvil.	1.60	Nail.	2.75
24*	Old College.	Building.	1.40		
25*	Fourth Story.	Building.	2.65	Fifth.	2.55
26*	Rope.			Ground.	4.00
27*	Fire.	Sherman Ave.	2.05	Engine.	1.80
28*	Danger.	Fire.	1.80	Escape.	3.70
29*	Minutes.	Hours.	1.55	Fire.	2.50
30*	Night.	Day.	.90	Dark.	3.07
31*	Before.	After.	1.27	Fisk.	3.60
32*	To write.	Speak.	3.37		
33*	Conceal.	Plain.	1.23	Knowledge.	2.30
34*	Hat.	Coat.	1.03	Black.	2.40
35*	Dread.	Fear.	1.55		
36*	Conceal.			Knowledge.	2.10
37*	Subconscious.	Conscious.	1.10	Stale.	1.80
38*	Ink.	Black.	1.30	Black.	1.40

In this table the significant stimulus words are marked with an asterisk, fourteen words having been first introduced to gain

the normal association time. Mr. H. was first tested and we soon came to the conclusion that he had not been in the room; this was further substantiated when Mr. O. was brought before the class and the length of his association time and the associations to the significant words were noted. At the conclusion of the experiment, before announcing the results to the class, every member was requested to indicate on a slip of paper which one of the two men had been in the room. The entire class, thirty-eight in all, was unanimous in its judgment that Mr. O. had been in the room. Only one of the thirty-eight thought that Mr. H. had also been in the room. We believe that the class drew its conclusions partly from the appearance of emotion in Mr. O., as well as from the manifestly delayed association time, when some of the significant words were given and by the nature of the associations themselves.

Subjoined will be found a table of the quantitative results of the experiments.

TABLE II.

QUANTITATIVE RESULTS OF EXPERIMENT I.

	Results for Mr. H—.		Results for Mr. O—.	
	Irrelevant Words.	Significant Words.	Irrelevant Words.	Significant Words.
Mean.	1.25	1.62	1.77	2.54
Mean Variation.	.29	.26	.56	.63
Difference in Means.	+ .37		+ .77	
Extremes { Long.	.85	.90	.87	1.05
Extremes { Short.	1.82	3.37	3.25	4.00
Maximum Range.	2.52		3.13	

The mean variation of Mr. H. on the irrelevant words was more than on the significant. While in the case of Mr. O. the mean variation for the irrelevant words was less than for the significant words, this being precisely what we expected.

We may sum up the grounds for our conclusion that Mr. O. had been in the room and had attempted to conceal as follows: (1) The association time for Mr. H. was not sufficiently lengthened in the case of the significant words to indicate a voluntary change of association. Mr. O.'s associations were manifestly delayed in the case of some of the significant words. Take, for instance, the two words *night* and *before*. These are from

the poem, 'Twas the Night before Christmas.' *Night* suggested the word *dark*, which was long in coming, possibly on account of the highly suggestible words *fire*, *danger* and *minute* just preceding, and when the stimulus word *before*, which naturally suggested *Christmas*, was given, Mr. O. changed this to *fisk*, thereby lengthening the time to 3.60 seconds. The same situation becomes apparent in the case of other significant words which can easily be selected by the reader. (2) Mr. O. gave a number of very significant associations in response to certain stimulus words. This, taken in connection with the highly delayed reactions, forms another basis for our conclusions. The word *rope* brought up the association *ground* in 4.00 seconds. An examination of the typewritten directions of this experiment under Number 6 where the sentence occurs, "This rope is long enough to reach 50 feet to the ground," will show just why this word was selected. (3) As we have already shown, Mr. O.'s mean variation for the significant words was considerably higher than for the irrelevant words, while the reverse was the case with Mr. H.

Experiment II.

In Experiment two, our object was three-fold: First, to discover which one of three subjects had performed a series of acts and was trying to conceal his relation to them; second, which one had performed the acts, and did not try to conceal; and third, which one knew nothing about them. Mr. J., Mr. S. and Mr. U. acted as subjects. We used the same room and the same directions as in Experiment Number I. Mr. W. was assistant and handed the subjects who were entirely ignorant of the nature of the experiment, the typewritten directions. When the subjects had fulfilled their parts, they returned to the lecture room one at a time and their associations were taken as is indicated in the following table:

The reader will readily see that this experiment was much more involved than the previous one, thereby increasing the difficulty of accurate diagnosis. The significant words caused a manifest delay in the association reaction time of Mr. S. This was not the case with Mr. U., while the slight difference

TABLE III.

RESULTS OF EXPERIMENT II.

No. of Word.	Stimulus Word.	Results for Mr. J.		Results for Mr. S.		Results for Mr. U.	
		Association Word.	Association Time.	Association Word.	Association Time.	Association Word.	Association Time.
1	Father.	Mother.	.92	Mother.	1.51	Papa.	1.02
2	To speak	To say.	1.42	To talk.		To say.	1.05
3	Grass.	Dog.		Green.	2.25	Green.	1.60
4	Sweet.	Sour.	.85	Sugar.	1.61	Sour.	1.23
5	Ten.	Twelve.	1.32	Twenty.	1.80	Twenty.	.77
6	Blue.	Black.	.97	Green.	1.80	Green.	1.37
7	Chair.	Table.	.93	Black.	2.00	Arm chair.	2.95
8	Girl.	Boy.	.82	Boy.	1.70	Light.	1.16
9	Supper.	Dinner.	.81	Dinner.	1.81	Dinner.	.75
10	To fly.	To see.	1.02	Bind.	2.15	To sail.	.85
11*	Christmas.	Thanksgiving.	1.35	Christmas Tree.	2.00	New Year.	1.02
12*	Interval.	Space.	1.56	Time.	2.55	Between.	1.14
13*	Skull.	Head.	1.37	White.	2.00	Bone.	.97
14*	Sleepy.	Sink.	1.08	Bed.	2.10	Tired.	.90
15*	Joseph.	James.	1.28	Jacob.	2.45		
16*	Ink.	Black.	1.39	Black.		Ink.	1.45
17*	Creature.	Man.	1.70	Bug.	2.00	Thing.	1.10
18*	Black.	Blue.	1.06	White.	1.20	White.	1.12
19*	Hammer.	Nail.	1.65	Black.	1.70	Nail.	1.25
20*	Old College.	Y. M. C. A.	1.80	University Hall.	2.76	New College.	1.15
21*	Fourth Story.	Fourth Floor.	1.12	" "	3.80	Third Story.	1.11
22*	Rope.	String.	.97	White Cord Line.	1.17	Cord.	1.16
23*	Fire.	Water.	1.32	Match.	1.67	Burn.	1.10
24*	Danger.	Signal.	1.21	River.	2.37	Tired.	.80
25*	Minutes.	Seconds.	.88	Sixty.	1.19	Seconds.	1.37
26*	Night.	Day.	.84	Black.	1.13	Rain.	1.38
27*	Before.	After.	.78	After.	1.47	After.	1.14
28*	To write.	To sing.	.79	Paper.	2.00	To left.	.70
29*	Conceal.	Hide.	1.10	Weapon.	2.25	Hide.	1.80
30*	Hat.	Cage.	1.50	Black.	1.57	Cob-web.	.90
31*	Dread.	Hate.	.89	Fear.	2.25	Tired	.99
32*	Conceal.	Hid.	1.17	Weapon.	1.29	Hide.	.96
33*	Jastrow.	Anything.	1.75	James.	2.70	Jastrow.	2.00
34*	Subconscious.	Ill.		Psychology.	2.25	Untie.	2.25
35*	Ink.	Black.		Black.	1.04	Red.	

*The significant words are indicated by an asterisk.

in time of Mr. J. could easily be accounted for on account of his unfamiliarity with the strange words. Further the reaction time of Mr. S. is on the whole greater than for Mr. J. or Mr. U., and finally, the variability in the reaction time of the significant words is greater for Mr. S. than for the other two. If we examine the association words of Mr. U. we find two very significant associations. The word *hat* brought up the word *cobweb*. Why? Was it not because the old crushed hat in the

room was dusty and covered with cobwebs? The word *ink* brought the response *red*, probably because the ink on the desk was red. When the word *Jastrow* was given to Mr. J. he gave the associated word *anything*, and the tone in which it was said indicated that he was unfamiliar with the word.

Before giving our decision to the class, each member was requested to write on a slip of paper what he believed to be the relation of each of the subjects to the experiment. Eight members were unanimous in their belief that Mr. U. had been in the room and was not trying to conceal it. Six thought Mr. S. had been in the room and was trying to conceal it, and six that Mr. J. had not been in the room.

Our conclusions were as follows: (1) Mr. J. had not been in the room; (2) Mr. S. had been in the room and tried hard to conceal it; and (3) Mr. U. performed the series of acts and did not try to conceal it. We were correct in our judgment. Below are given the quantitative results of Experiment II.; in this the mean variation in the significant words stands out very prominently in the case of Mr. S.

TABLE IV.

QUANTITATIVE RESULTS OF EXPERIMENT II.

	Results for Mr. J.		Results for Mr. S.		Results for Mr. U.	
	Irrelevant Words.	Significant Words.	Irrelevant Words.	Significant Words.	Irrelevant Words.	Significant Words.
Mean.	1.01	1.24	1.85	1.95	1.23	1.21
Mean Variation.	.16+	.26+	.19+	.47+	.41	.26+
Difference in Means.	+0.23		+0.10		+0.02	
Extremes { Short.	.81	.78	1.61	1.04	.75	.70
Extremes { Long.	1.42	1.80	2.25	3.80	2.95	2.25
Maximum Range.	1.02		2.76		2.25	

EXPERIMENT III.

Having been successful in the previous enumerated trials and others, we determined to further restrict any advantage which the experimenters might have over the subject. Two men, Mr. S. and Mr. O., both of whom had knowledge as to the nature of the association reaction method, were selected, and they were given the following options: (1) both might perform the series of acts according to instructions; (2) either one could

perform them; (3) neither need necessarily perform them; (4) after having chosen what they would do they were allowed to conceal, or not to conceal, their individual relation to the experiment.

In a drawer of a table in the psychological laboratory, the following things were placed; three bottles of ink (carmine, green and violet), two pieces of glass (red and blue), a one-pound iron weight, a handkerchief scented with asafœtida, a copy of *The Psychology of Advertising*, by Walter D. Scott, and *The Native Tribes of Central Australia*, by Spencer and Gillen.

The written instructions were as follows:

"1. Take the book, *The Psychology of Advertising*. Who is the author? Turn to page 44. Read the advertisement on that page carefully."

"2. There are three bottles of ink in the drawer. Notice carefully the color. Are the bottles full or empty?"

"3. Take the book by Spencer and Gillen, *The Native Tribes of Central Australia*." This is a large book. How many pages has it? Turn to page 33 and notice the old man. Also turn to page 47.

Is not she a winsome lass? It is too bad that she has lost one tooth!"

Mr. S. was first examined and in order to increase the probability that our conclusions were correct the list of words was given a second time. Mr. O. was then examined in the same way. The results are indicated in Table V.

A comparison of Mr. O.'s figures in Table V. with those of Table I. shows that, while his figures are somewhat higher than those of some other normal subjects, yet the difference between the means in Experiment III. is much less than in the first experiment, in which he tried to conceal. Moreover, the mean variation both for the irrelevant and significant words is in general much lower than in the previous experiment. It will also be seen by a comparison of the quantitative results of these experiments that his maximum range is much higher in the first than in the l[illegible] The variability, however, of his reactions was not a suf[illegible]asis for concluding that he was trying to con-

TABLE V.

RESULTS OF EXPERIMENT III.

No. of Word.	Stimulus Word.	Results for Mr. S.				Results for Mr. O.			
		Association Word (1st Time).	Ass'n Time (1st).	Association Word (2d Time).	Ass'n Time (2d).	Association Word (1st Time).	Ass'n Time (1st).	Association Word (2d Time).	Ass'n Time (2d).
1	House.	You.	.65	Dinner.	.50	Barn.	1.60	Barn.	3.50
2	*Man.	Old.	.99	Five.	.45	Woman.	1.60	Woman.	1.70
3	Boy.	Bugs.	.70	You.	.85	Girl.	1.40	Girl.	1.32
4	Weber.	This.	1.19	Heavy.	.53	Cold.	1.10	Cold.	1.60
5	Law.	This.	1.41	Long.	.56	School.	2.95	School.	1.80
6	Hypnotism.	This.	1.65	You.	1.08	Eddy.	2.75	Eddy.	1.70
7	*Girl.	Yes.	1.25	Outside.	.87			Barn.	1.50
8	University.	North-western.	1.12	Caught.	.75	Ground.	2.83	Ground.	1.70
9	April.	May.	1.14	North-western.	.90	Second.	1.80	Second.	
10	*Ink.	Yes.	1.40	Late.	.60	Black.	1.54	Black.	1.60
11	*Book.	Black.	1.62	Old.	.25	Black.	2.75	Black.	1.24
12	*Psychology.	Thorndike.	1.65	This.	1.12	Class.	1.80	Class.	2.70
13	*Woman.	Old.	.77	Cold.	.76	Man.	1.80	Man.	1.60
14	*Hairy.	Hairy.	1.37	Forget.	.83			Man.	1.80
15	*Winsome.	No.	1.43	Metric.	.56	Man.	2.65	Man.	
16	External.	Yes.	1.50	Lass.	.90	Skin.	2.60	Skin.	1.17
17	*Green.	Fisk.	.60	Blue.	1.00	Grass.	1.80	Grass.	1.42
18	*Violet.	Yes.	1.47	Any.	1.00	Pansy.	1.70	Pansy.	1.66
19	*Red.	Carmine.	.87	Carmine.	.95	Black.	1.70	Black.	1.60
20	*Odor.	Carmine.	.79	Caterpillar.	1.00	Stink.	2.25	Stink.	1.50
21	Library.	Russell.	.72	Coxcomb.	.76	Lunt.	2.85	Lunt.	1.50
22	*Central.	This.	.83	Switch.	.45	Street.		Street.	1.27
23	*Native.	This.	.80	You.	1.10	Africa.	1.70	Africa.	1.62
24	*Man.	Nice.	.97	Old.	.77			Woman.	1.50
25	*Old.	Yes.	1.20	Woman.	.80	Man.	1.80	Man.	1.70
26	*Skin.	Black.	.90	Drawer.	.52	Black.	1.33	Man.	1.80
27	Six Hundred.	Yes.	1.07		1.15			Seven Hundred.	1.80
28	*Skin.	Ride.	.60	Wry.	.65	Black.	1.70	Man.	1.70
29	*Hair.	Black.	.80	This.	.64	Brown.	1.80	Brown.	1.57
30	*Tooth.	Buds.	.71	Buds.	1.10	White.		White.	1.80
31	*Page.	Three.	.85	Yes.	.47	Book.	1.80	Book.	1.80
32	*Weight.	Heavy.	.89			Iron.	1.80	Iron.	1.33
33	*Glass.	Yes.	1.04	Heavy.	.76	Bottle.		Bottle.	1.80
34	*Pound.	Heavy.	.96	Kilo.	.80	Ounce.	1.80	Ounce.	1.80
35	*Fat.	Kilo.	.76	Yes.	1.00	Man.	1.80	Man.	1.80
36	*Cook.	Meter.	.72	Quick.	.42	Mrs. Rhadge.	1.80	Mrs. Rhadge.	1.80

ceal. It will be remembered that there was a handkerchief scented with asafœdita in the drawer. When the word *odor* was given, O. gave the association *stink*. Again, when the stimulus word *weight* was given, he answered, *iron*. This, perhaps, because of the iron weight in the drawer. And when the word *hairy* was given the reply was *man*. This would ap-

pear to be on account of the picture of a hairy man on page 33 of the book, *The Native Tribes of Central Australia.*

In the case of Mr. S. it is at once apparent that for some reason he repeated words in giving associations which were manifestly not prompted by the stimulus word. This is shown by the short reaction time. The word *red* called up the word *carmine*, which was the name on one of the bottles of ink. When *winsome* was given he gave the word *metric* in .56 of a

TABLE VI.

QUANTITATIVE RESULTS OF EXPERIMENT III.

	Results Mr. S.				Results Mr. O.			
	(1st Time) Irrelevant Words.	(1st Time) Significant Words.	Reproduction Irrelevant Words.	Reproduction Significant Words.	(1st Time) Irrelevant Words	(1st Time) Significant Words.	Reproduction Irrelevant Words.	Reproduction Significant Words.
Mean.	1.01	1.11	.75	.76	1.84	2.21	1.66	1.78
Mean Variation.	.26	.20	.26	.15	.22	.65	.18	.38
Difference in Means.	+.10		+.01		+.37		+.12	
Extremes { Long.	1.65	1.65	1.08	1.12	2.95	2.75	3.50	2.70
Extremes { Short.	.65	.60	.50	.25	1.10	1.33	1.17	1.24
Maximum Range.	1.05		.87		1.85		2.33	

second. This word he evidently had in mind as his next association word. *Winsome* was followed by the stimulus word *external*, the association given for this being *lass*, we had reason to believe that this was the actual association called up by the word *winsome.* By turning to the written directions the phrase, 'winsome lass' will be found. An examination of the quantitative results of the experiment shows that Mr. S. had association words in mind before the stimulus word was given. This has greatly reduced the average time, both for the irrelevant and the significant words. The difference in the association time of the irrelevant and the significant words of Mr. O. indicates the emotional tendency of a significant stimulus word, whether or not the subject tried to alter his first associations.

Our judgment on this last experiment was (1) that both Mr. O. and Mr. S. had been in the room and had performed the series of acts, (2) that Mr. O. did not try to conceal, (3) that Mr. S. made an attempt to conceal.

Our conclusions regarding the general feasibility of the association reaction method in mental diagnosis are as follows:

I. Accurate judgment is reasonably certain, in event the situation is so controlled that the diagnosis take the simple form of determining between two possible alternatives, the exact motive of which is definitely known by the experimenter; our results in this regard have been in substantial agreement with those given by Messrs. Yerkes and Berry.[1]

II. In such cases it is probable that the diagnosis will be so simple that a third party who has observed the experiment will be in a position to draw right conclusions.

III. Knowledge of the association reaction method on the part of the subject, though he attempts to utilize it in concealing his relation to the experiment, does not make a correct diagnosis impossible.

IV. The difficulty of accurate diagnosis increases in proportion as the advantage which the experimenter has over the subject is gradually restricted, and the number of possible diagnoses increased. We see no reason why the situation might not conceivably be so complicated that accurate diagnosis would *ipso facto* be impossible.

[1] Cf. *The American Journal of Psychology*, January, 1909, p. 226.

BINOCULAR RIVALRY.

BY B. B. BREESE,
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In a former report on binocular rivalry[1] the average length of the normal rivalry phases for 10 mm. squares was reported to be 1.89 seconds with an average variation of .5 second. In the experiments upon which this report was based the stimuli — red and green squares with black lines running diagonally across them — were mounted upon a stereoscopic slide and placed in the stereoscope so that the red square was presented to the right eye and the green square to the left eye. The background of both squares was a uniform black. The length of time each field remained in consciousness was registered by means of electric keys connected with the recording pens of a kymograph drum. The observations were made upon myself ten years ago.

Within the last year I have repeated the experiments under the same conditions except that the squares were crossed by vertical and horizontal instead of by diagonal lines; vertical on the green and horizontal on the red squares. These squares were mounted as before on a uniformly black stereoscopic slide. Registration of the length of the phases of the rivalry was made upon a revolving drum by means of keys electrically connected with pens, one for each hand. The right key was pressed down when the right (red) field was in consciousness and the left key when the left (green) field was in consciousness. The kymograph drum carried a tuning fork marker which registered the time in hundredths of seconds.

Three hundred changes in the rivalry showed an average phase length of 1.84 seconds with an average variation of .5[2]

[1] 'On Inhibition,' Vol. III., No. 1, Monograph Supplement, PSYCHOLOGICAL REVIEW.

[2] There is considerable variation in the lengths of the rivalry phases for different individuals under the same conditions. This was found to be true in previous experiments. See also 'A Study of Retinal Rivalry in the After-image,' by Alma de Vries and Margaret F. Washburn, in the January number of the *American Journal of Psychology*, p. 131.

second. Between 54 and 55 changes occurred in the red and green fields during every 100 seconds, practically the same rate of fluctuation in the rivalry as that of ten years ago. The following account gives the time of the rivalry phases and rate of fluctuation under new conditions not yet reported upon.

THE EFFECT OF VARIATION IN THE SIZE OF THE STIMULI.

Five slides were prepared like the one used in the previous experiment except that they varied in size from 3 mm. to 30 mm. One hundred changes were recorded in each case. Tables I. and II. give the results

TABLE I.

Size of Squares.	Length of Time of 100 Changes.	Average Length of Phase.
3 mm. squares.	317 seconds.	3.17 seconds.
8 " "	240 "	2.40 "
10 " "	184 "	1.84 "
20 " "	152 "	1.52 "
30 " "	130 "	1.30 "

The rate of fluctuation per 100 seconds is given in Table II.

TABLE II.

3 mm. squares.	31–32 changes in 100 seconds.
8 " "	41–42 " " " "
10 " "	54–55 " " " "
20 " "	65–66 " " " "
30 " "	76–77 " " " "

In one of the experiments already reported the stimuli were of different sizes for the two eyes. A 10 mm. square was presented to the right eye, a 5 mm. square to the left eye. This change in the size of the stimuli resulted in lengthening the average phase for the smaller square by .34 of a second. The normal rivalry, when 10 mm. squares were used for both eyes, showed 53–54 changes in 100 seconds and an average phase length of 1.89. But when one of the squares was reduced to 5 mm., the rivalry was reduced to 44–45 changes in 100 seconds and the phase corresponding to the smaller square was lengthened to 2.23 seconds while the phase for the larger square remained the same.

The effect of increasing the size of the squares is analogous to that which resulted from increasing the light intensities of

the stimulating squares. Experiments with the 10 mm. squares showed that the rate of fluctuation in the rivalry increased and decreased with the increase and decrease of the intensity of the lights used to illuminate the squares.[1] These lights varied from a very dim light just sufficient to make the lines upon the squares clearly perceptible to that of a 100 c.p. arc light. Table III. is compiled from the data of previous experiments.

TABLE III.

Light Intensities.	Rate per 100 Seconds.	Phase Length.
Dim light.	24–25	4.24 seconds.
16 c.p. at 400 cm. (Inc.).	46–47	2.15 "
16 c.p. " 50 " "	58–59	1.70 "
80 c.p. " " " "	67–68	1.48 "
100 c.p. " " " (Arc).	83–84	1.20 "

In the data given above the light intensities for each eye were equal in every case. But if the squares were unequally lighted, it was found that the phase length corresponding to the brighter square was lengthened instead of shortened. In every case where there was an equal increase in the light intensities of both squares there was an increase in the fluctuation of the rivalry and a decrease in the phase lengths, but when one square only was increased in light intensity, the rate of fluctuation was decreased, due to the increase in the phase length of the brighter square.

THE EFFECT OF DISTINCTNESS OF THE IMAGES UPON RIVALRY.

In the above experiments the stereoscopic slides were so adjusted that the retinal images were brought to a sharp focus on the retinæ. In order to determine what effect a change in the distinctness of the images would have upon the rivalry 400 changes were measured, 200 in focus and 200 out of focus. The slide with the 10 mm. squares was used.

IN FOCUS.

200 changes in 363 seconds.
Average length of phase 1.81 seconds.
Average variation .5 second.
55–56 changes in 100 seconds.

[1] 'On Inhibition,' page 39, Sec. 9.

Pushing the slide out 70 mm. from its position of sharpest focus, or until the black lines on the squares were just distinguishable, the following results were obtained:

OUT OF FOCUS.

200 changes in 748 seconds.
Average length of phase 3.74 seconds.
Average variation .5 + second.
26–27 changes in 100 seconds.

PERIPHERAL RIVALRY.

So far, only the rivalry of the central parts of the retinæ has been considered. For comparison of central with that of peripheral rivalry the slide with the 8 mm. squares was used. When placed in position in the stereoscope for clearest vision the slide was found to be 200 mm. from the principal foci of the eyes. In order to keep the distances from all parts of the slide to the retinæ uniform when the fixation points were changed, each half of the slide was mounted upon sections of spheres whose radii were 200 mm. The 8 mm. squares were each placed in the central parts of the sections and around each were placed eight fixation points, two to the right, two above, two to the left, and two below. All the points to the right and left of the squares were in the horizontal plane which cuts the eyes into upper and lower halves, and all the points above and below were placed in the median planes of each eye when they fixated the centers of the squares. The arrangement was such that there was a fixation point directly to the right, directly above, directly to the left and directly below the center of each square and at an angular distance of 3.6° from the centers. Similarly a second set of fixation points was placed at a distance of 7.2° from the centers.

When placed in the stereoscope the red square and the eight fixation points of the right field and the green square and eight fixation points of the left field were superimposed. Fixation of the points to the right resulted in projecting the images of the squares upon the temporal half of the right retina and upon the nasal half of the left retina, the right square upon the right eye and the left square upon the left eye respectively. If the first point were fixated the images of the squares were

approximately 3.6° from the foveæ and upon corresponding areas of the retinal surfaces. The rivalry then took place upon the peripheral parts of the retinæ. If the second point to the right were fixated then the images of the squares were approximately 7.2° from the foveæ. When the points to the left were fixated the images were thrown upon the left halves of the eyes; when the points above were fixated the images were thrown upon the upper halves; when the points below were fixated the images were on the lower halves of the eyes and at the distances from the foveæ represented by the distances of the fixation points from the centers of the squares. This gave an opportunity to measure eight sets of rivalry images upon the peripheral parts of the retinæ, and to compare the rates of rivalry and the lengths of the phases with that upon the central parts of the eyes. The rivalry was found in every case to be very much slower on the periphery and consequently the phases of the fluctuation were very much lengthened. The phase length was more than doubled while the rapidity of the fluctuations was less than one half that of normal central rivalry. The zones further from the foveæ gave a slower rivalry rate than the zones nearer the fovea. Table IV. gives the results of this experiment.

TABLE IV.

Central Rivalry, 8 mm. Squares.

		Number of Changes Recorded.	Total Time of Changes.	Average Length of Phases.
		62	149 seconds.	2.44 seconds.
Peripheral Rivalry, 8 mm. Squares.				
Fixation points 3.6° from center of squares.	Right.	28	134.8 seconds.	4.80 seconds.
	Up.	40	236.9 "	5.92 "
	Left.	28	120.2 "	4.30 "
	Down.	40	208.7 "	5.22 "
Fixation points 7.2° from center of squares.	Right.	24	121.5 seconds.	5.06 seconds.
	Up.	22	131.6 "	5.98 "
	Left.	22	125.7 "	5.71 "
	Down.	22	127.2 "	5.78 "

The average variation for the central rivalry was .5 second; for peripheral rivalry it was 1.1 seconds. The general average

of the length of the phases for the zones 3.6° from the foveæ was 5.05 seconds, while that for the zones 7.2° from the foveæ was 5.64 seconds. The rate of rivalry per 100 seconds was as follows:

TABLE V.

Central Rivalry.			40–41 changes in 100 seconds.
Peripheral Rivalry.	Fixation point 3.6° from center of squares.	Right.	20–21 " " " "
		Up.	16–17 " " " "
		Left.	20–21 " " " "
		Down.	19–20 " " " "
	Fixation point 7.2° from center of squares.	Right.	19–20 " " " "
		Up.	15–16 " " " "
		Left.	17–18 " " " "
		Down.	17–18 " " " "

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF WELLESLEY COLLEGE.

(COMMUNICATED BY PROFESSOR ELEANOR A. McC. GAMBLE.)

I.

Intensity as a Criterion in Estimating the Distance of Sounds.

BY E. A. McC. GAMBLE.

The purpose of this study was to find evidence for or against the ordinary assertion that the distance of a sound is estimated mainly on the basis of its intensity. The investigation falls into two divisions. In the first division the evidence was sought by an indirect method. The experiments constituted an attempt to determine the just noticeable divergences from a number of standard distances. The argument on which the work was based is as follows:

1. The intensity of a sound varies inversely as the square of the distance. If the relative distance of two sounds is expressed by the ratio 9:16, then, other things being equal, their relative intensity must be expressed by the ratio 4:3.

2. If the relative distance of sounds is judged in terms of their intensity, then a just noticeable difference in distance may be expected to imply a just noticeable difference in intensity. This means that if a sound at a distance of 9 feet is just noticeably nearer than the same sound — *i. e.*, a sound produced by the same stimulus — at a distance of 16 feet, then the just noticeable difference in intensity must be one third of the intensity of the weaker stimulus. It should be noted, however, that it is conceivable that intensity is the main criterion in judging only gross differences in distance, and that variation of overtones is highly important in judging liminal differences.

3. If Weber's law holds for sounds in general, then a just noticeable variation from the intensity of two or more standard sounds produced by the same stimulus must be approximately

the same fraction of the total intensity of these standard sounds. If we grant all these premises, then we must infer that whenever we find a just perceptible difference between distances of the same sound, we shall find that the intensities as determined by these distances always bear about the same ratio to one another. Several investigators have found that one third is the 'Weber's law fraction' for the noise of small falling bodies. Let us suppose that this fraction holds for sounds in general, whether noises, tones of different pitches, or clangs. Then one sound will always be just noticeably nearer and louder than another when the intensity ratio is approximately 4 : 3. Whenever we find a just noticeable difference in distance, we shall find that the distances are as 9 is to 16 and that the intensities are as 4 is to 3.

The fraction one third has been taken only for purposes of illustration. It is more than probable that it does not apply to sounds at large. Let us then represent two just noticeably different distances of the same sound by m and n. Now on the assumptions of the foregoing argument, one will find that the ratio which holds between m^2 and n^2 always holds between the squares of any two just noticeably different distances of this sound. , Therefore, if in various instances of just perceptible difference in distance, we find no such equality of ratios, then either Weber's law does not apply to the sound-stimulus used, or else intensity is not the main criterion in estimating liminal differences in its distance, or else the validity both of Weber's law and of the intensity-criterion are ruled out in the particular case. If, on the other hand, the equality of ratios is found to hold repeatedly, then there is a strong presumption both that Weber's law does apply to the stimulus in question, and that intensity is indeed the main criterion in estimating liminal differences in the distance of this particular sound. Of course, a negative conclusion will be warranted only if the experimental conditions are good or if the results are so numerous that the effects of accidental variations in the conditions may be supposed to cancel one another.

In the second division of the experiments the method was more direct. The subjects, who knew little or nothing of the

object of the experiments, were required to describe repeatedly the difference in two sounds, which varied sometimes in initial intensity and sometimes in distance, or which were altered in intensity when they were supposed to be altered in distance. The purpose of these experiments was to determine the degree to which the subjects were likely to confuse one difference with the other. For brevity, the experiments of the first division will be called the 'Weber's law experiments,' and those of the second division will be called the 'confusion experiments.'

Experiments of both divisions were made in the academic years 1897–1898, 1898–1899 and 1899–1900.[1] Throughout the experiments the sounds were given with a telephone receiver and the distance from the subject's ear was measured upon a board supported at such a height that the opening of the receiver when held close above it — with only the experimenter's fifth finger inserted between receiver and board — could be approximately on a level with the opening of the ear of the subject whose chair could be raised or lowered according to her height.

In the first year of the experiments, this board was about two inches wide and was raised on supports about 16 inches from a table 36 inches wide. In the last two years the measuring-board was only half an inch thick, had a bevelled edge graduated in half-centimeters, and was held, edge-up, by slender supports which rose from the floor. The room is which the experiments were made is 47 feet long and 15½ feet wide. The subject's end of the board was about 10 feet from one end of the room and the board ran parallel with the longer walls of the room and practically in the center crosswise. From this end of the room all furniture unnecessary to the experiments was cleared away. The room was reasonably but not ideally quiet. Rude as the conditions thus indicated may seem, by far the most serious drawback to the experimental conditions consisted in the nature of the sound itself. The sound used was not a telephone-click; the click was considered too weak and irregular ('sputtering') for the purpose. The sound employed

[1] The experiments were made in the three successive years by Miss Louise S. McDowell, Miss Amy G. Whitney and Miss Inez Mathews, who were all students in a second-year course in psychology. The work was directed at different times by Professor Calkins, by Dr. James E. Lough and by the writer.

was a 'musical tone' produced by passing the alternating lighting-current of the college, or a secondary current induced by this current, through the telephone receiver. The coil on the magnet of a discarded piece of apparatus was thrown into the circuit by way of resistance. When the primary current was used, the opening of the receiver was plugged with cotton to reduce the loudness of the sound. The secondary current produced a sound which erred in the direction of being too weak, but the intensity could be further reduced at will by sliding out the induction-coil. The great defect in the experimental arrangements consisted in the fact that the intensity of the sound varied considerably from one sitting to another according to the number of electric lamps through which the current was passing. A minor difficulty consisted in the 'humming' of the induction-coil. In the first year of the experiments no induction-coil was used; diminution in the initial intensity of the sound (*i. e.*, diminution in its loudness near its source and not as determined by distance) were produced by screening the receiver with the hand. In the second year the induction-coil was used only in the confusion experiments. In half of these experiments the initial intensity of the sound was altered by sliding out the coil; in the other half the screening-method was used. In the third year, the induction-coil was used in all the experiments because in consequence of a change in the dynamo supplying the alternating current, the sound produced by the primary current had altered to a harsh bray. In all cases, the experimenter made and broke the circuit by means of a push-button on a shunt.

Throughout the experiments reasonable precautions were taken to cancel the effect of the time-error, the expectation-error, and the like. (The experiments were scarcely elaborate enough to merit a detailed account of program.) At least in the last two years, the sounds to be compared were given two seconds apart and the subject was required always to judge the sound with reference to the first as a standard. The subjects with one exception were all students in a first-year course in psychology. In the confusion experiments they were blindfolded, but in the other experiments they were simply required 'not to look.'

The Weber's law experiments of the first two years led to no

very definite outcome. The work of these two years consisted in skirmishing to hit upon the divergence from a number of standard distances which would give 80 per cent. of right cases in comparing the two distances. Perforce, a number of different subjects were used — three in the first year and four in the second and only one of them trained — because no one subject was available for extended work. With each subject several different distances were used as standards, ten different distances were compared with each standard, and only ten comparisons were made with each pair of distances. (Comparisons of the same standard with different distances were interspersed with one another.) In view, on the one hand, of the variations which must arise under experimental conditions of so rough a nature, and in view, on the other, of the scattering of the experiments over so many subjects and distances, it is scarcely surprising that little regularity appears in the figures obtained. The results of the second year are rather less regular than those of the first. The latter may be summarized as follows, if one averages the results of the three subjects and if one assumes that about 80 per cent. of right judgments indicates a liminal difference between two stimuli:

Standard distances in cm.:	20	30	40	60	80	100	120	140	200	300
Ratio between intensity at standard and distance just noticeably greater:	$\frac{100}{69}$	$\frac{100}{74}$	$\frac{100}{86}$	$\frac{100}{76}$	$\frac{100}{61}$	$\frac{100}{82}$	$\frac{100}{82}$	$\frac{100}{72}$	$\frac{100}{86}$	$\frac{100}{80}$
Distance just noticeably less:	$\frac{100}{111}$	$\frac{100}{135}$	$\frac{100}{111}$	$\frac{100}{111}$	$\frac{100}{120}$	$\frac{100}{145}$	$\frac{100}{118}$	$\frac{100}{130}$	$\frac{100}{125}$	$\frac{100}{125}$

These figures look very much like the sort of results which might very well be obtained from unpracticed subjects, under rough conditions, if Weber's law applied to the stimulus and the fraction were about one fifth.[1]

In the third year only two subjects were employed — L., a student in a second-year course in psychology, and G., the writer. The plan of the experiments was to find a pair of distances which would give, when compared, from about 75 to 80 per cent. of right cases, and then to work with another or other

[1] M. Wien found the fraction one fifth to hold good for the tone *a* at about 220 vibrations. For the corresponding *e'*, he found the fraction to be one sixth and for the corresponding *a'*, he found the fraction to be one eighth. This statement is made on the authority of Ebbinghaus, *Grundzüge der Psychologie*, 1905, p. 302.

pairs of distances which would involve the same ratio between the intensities of the sounds. The results of these experiments are given in the following table:

RESULTS OF THIRD YEAR OF EXPERIMENTS IN DETERMINING LIMINAL DIFFERENCES IN SOUND DISTANCES.

Subject.	Set of Experiments.	Distances Compared, cm.	Ratio of Corresponding Intensities.	Number of Comparisons.	Right Cases, Per Cent.
G.	1	20 and 25	156 : 100	224	91
	2	20 " 22.5	127 : 100	250	89
	3	30 " 35	136 : 100	425	89
	4	30 " 33	121 : 100	475	86
	5	30 " 32	114 : 100	400	66
	6	30 " 32.5	117 : 100	475	76
L.	1	36 and 43	143 : 100	825	71
	2	36 " 46	163 : 100	150	99
	3	36 " 43.5	146 : 100	825	74
	4	26 " 31	142 : 100	725	72
	5	46 " 55.5	146 : 100	325	77

In the case of G., the effect of practice made the '80 per cent. point' hard to find. A difference in distance which at first promised to give far less than 80 per cent. of right judgments would toward the end of a set of comparisons yield far more. Thus, only one difference was finally demonstrated to be liminal, viz., the difference between 30 and 32.5 cm. That this difference was really liminal is shown by the fact that either an excess or a lack of half a centimeter made a great difference in the number of right judgments obtained. It is noteworthy that the intensity-difference implied by this liminal distance-difference is about one fifth of the smaller stimulus-intensity.

The results obtained from L. certainly seem, in so far as they go, to prove the point at issue. In two cases in which the intensity-ratio between the sounds compared was approximately the same, the percentage of right cases was almost exactly the same, and in two other cases in which the intensity-ratio was exactly the same the percentage of right cases was approximately the same. Moreover, the number of comparisons in each case was respectably large. The fraction which measured the just noticeable difference was, however, very large, amounting to two fifths of the smaller intensity.

On the whole the results of the first division of the experiments suggest although they do not prove, that the estimation of sound-intensities in general follows Weber's law, and also forms the basis of the estimation of liminal differences in distance.

The confusion experiments were both simpler and much more fruitful of results. Their conduct may conveniently be described in connection with the following table which summarizes the data obtained:

RESULTS OF 'CONFUSION EXPERIMENTS.'

Year 1.

Difference in Stimuli.	Cases.	Distance in Cm.											
		30			60			120			240		
		Judgments Per Cent.			Judgments Per Cent.			Judgments Per Cent.			Judgments Per Cent.		
		N	*F*	*E*	*N*	*F*	*E*	*N*	*F*	*E*	*N*	*F*	*E*
Second sound louder.	30	87	7	7	80	7	13	73	13	13	40	37	23
Second sound softer.	30	—	100	—	—	97	3	17	83	10	37	53	10

Year 2.

Relation of Stimuli.	Method.															
	Screening.								Use of Induction Coil.							
	Distance.								Distance.							
	15 cm.					30 cm.			15 cm.				30 cm.			
	Cases.	Judgments Per Cent.				Cases.	Judgments Per Cent.				Cases.	Judgments Per Cent.				Cases.	Judgments Per Cent.			
		N	*F*	*E*	*U*		*N*	*F*	*E*	*U*		*N*	*F*	*E*	*U*		*N*	*F*	*E*	*U*
Second sound louder.	152	73	5	22	—	153	72	9	19	—	162	65	7	27	1	142	65	8	26	1
Second sound softer.	125	6	78	14	1	124	6	80	12	2	132	9	77	14	—	116	5	84	10	—
Both sounds loud.	31	26	3	71	—	32	16	3	81	—	36	19	8	72	—	27	11	15	74	—
Both sounds soft.	41	15	5	81	—	42	12	10	79	—	45	4	20	73	2	139	10	21	69	—

Year 3.

Difference in Stimuli.	Cases.	Judgments Per Cent.					
		N	*F*	*L*	*S*	*E*	*U*
Second sound nearer.	893	22	5	29	8	32	4
Second sound farther.	1159	5	28	6	29	27	5
Second sound louder.	509	21	2	35	8	30	4
Second sound softer.	712	1	28	2	52	13	3

The only abbreviations which need explanation are the initials in the columns under 'judgments per cent.' *N* means that in

a certain percentage of a given set of comparisons (of which the number is given under 'cases'), the second sound was judged to be the 'nearer' of the two. In the same way, *F* stands for 'farther,' *L* for 'louder,' *S* for 'softer,' *E* for 'equal' or 'same,' and *U* for 'uncertain.' All cases in which for any reason the subject failed to pass judgment are gathered under *U*. In the figures for each of the three years, the results obtained from all the subjects are massed (not averaged) as if they had been obtained from one subject. In the first year, the subjects numbered three, and sixty cases — twenty for each subject — were obtained with each distance — thirty with the second sound louder and thirty with it softer. In the second year, the subjects numbered fifteen and each subject made about twenty-five comparisons with each method of altering the initial intensity of the sound (screening and use of the induction-coil) at each distance — about one hundred comparisons in all. In the third year of work, thirty-two subjects were used, and each subject made about one hundred comparisons. In this year no attempt was made to compare the results which might be obtained at different standard distances. The subjects of the confusion experiments were all first-year students of psychology, but those of the first year of work were the same three who had served as subjects in the experiments of the Weber's law division.

In the first two years of the experiments, the subjects were led to think that only the distance of the sound would be varied, whereas, if any difference at all were made in the stimulus, only the initial intensity of the sound was actually varied. As important a point as any which the figures bring out is that the subjects did not detect the imposition which was practiced upon them. (To this rule there are one or two exceptions which are of little practical importance since the subjects' misgivings, which never amounted to more than suspicion, were due to same carelessness or misadventure on the part of the experimenter.) The figures show that in the great majority of cases, difference in intensity produced the impression of difference in distance — in such wise that the louder sounds were interpreted as the nearer — and that equality of intensity produced the im-

pression of equality in distance. Although the sounds were all really equal in the respect in which the subjects judged some of them to be different, and although in the second year some pairs of sounds were given which were really equal in all respects and were judged to be equal, yet no preponderating tendency appears to err in the direction of passing too many equality-judgments. As regards the experiments of the second year, it should be noted incidentally that a greater change of intensity seems to have been produced by screening the telephone than by sliding out the coil. The coil was moved from 3 to 5 cm. according to the strength of the current on the particular day. Since the subjects did not detect the very simple ruse employed, the greater number of right cases obtained with the screening-method can scarcely be laid to any peculiar muffling of the sound.

In the first two years the subjects were under the influence of suggestion when they interpreted differences in intensity as differences in distance. The effect of suggestion might conceivably be great enough to make the subject's image different 'distance-qualities,' if such marks ever exist, with different sound-intensities. In the third year, the subjects were entirely free from the effect of suggestion, as regards the point at issue. They were required simply to tell *how* the sounds in the pairs given them for comparison differed. It was suggested merely that these sounds might differ in distance, in loudness, or in pitch. The statement was purposely made in such a form that the unreflecting subject could think that 'the same' sound might be nearer without being louder. As a matter of fact, the difference was sometimes one of distance, and sometimes one of initial intensity, as controlled by the use of the induction-coil. The extent to which the coil was pulled out and the distances at which the sounds were given differed somewhat from one sitting to another according to the strength of the primary current, but at the same sitting only two distances and two intensities were compared. The one hundred comparisons demanded of each subject were ordinarily made at one sitting. The distance at which the nearer sound was given rarely exceeded 30 cm. The experimenter meant to work with superliminal dif-

ferences both of distance and of intensity, but the figures indicate that the differences were in general not more than liminal.

The third part of the table shows that the judgments of 'nearer' and 'louder,' and of 'farther' and 'softer' were practically interchangeable. The subjects showed a marked tendency, however, to say more often that a sound was louder when it was louder only in virtue of being nearer, than to say that it was nearer when it was merely louder, and so also, *mutatis mutandis*, with the judgments of 'softer' and 'farther.' This fact may be interpreted in three different ways: (1) If one beg the question at issue in this investigation as a whole, one may say that when the initial intensity and the distance of a sound are both unknown, one's attention dwells upon intensity just because one is more accustomed to making intensity the clue to distance than to making distance the clue to intensity. In view of the whole trend of the confusion experiments this seems to the writer the natural explanation of the tendency towards judgments of 'louder' and 'softer,' and the tendency itself seems to be a striking confirmation of the ordinary belief which is here in question. The fact that there were any judgments at all of 'nearer' and 'farther' is, in view of the results of the first two years, sufficiently explained by suggestion. (2) If, however, one believes in distance-qualities, one may say that the subject is more likely to overlook the difference in such qualities than to imagine one. (3) Finally, the tendency in question may (conceivably) be due to the fact that the subjects were reflecting enough to realize, at least dimly, that nearness implies loudness in a way that loudness does not imply nearness, so that the intensity-judgment has a double chance of being right. There are, however, few recorded remarks or other data which lead one to believe that the subjects at large clearly distinguished between the loudness of a sound as determined by its distance and its initial loudness. The failure of our subjects to make this distinction must not be interpreted as telling against the practical value of intensity as a clue to distance. One may associate place-images with intensities for practical purposes in ordinary life — as, for example, when one is estimating the distances of a railway-train which one wishes to catch — and

yet, in spite of these serviceable associations, one may fail to think clearly about the two conditions of intensity on occasions when intensity and distance are alike unknown and are equally uninteresting to the natural man.

Three additional remarks must be made: (1) In these records — for the confusion experiments of the last year — there is a sprinkling of cases in which the same sound was judged to be both nearer and louder or farther and softer, and a still smaller number of cases — about a dozen out of 3,273 — in which the same sound was called both nearer and softer or farther and louder. These double judgments are reckoned in the table as if the first judgment expressed had been the only one. They may be interpreted either for or against the assumption of a sharp distinction on the part of the subjects between the two conditions of loudness.

(2) Differences in pitch or musical quality were very rarely alleged by the subjects — not nearly so often indeed as differences in duration, which were purely accidental. No correlation can be made out between the pitch-differences mentioned and differences in distance.

(3) Curiously enough, throughout the confusion experiments of the three years, the number of right and of pseudo-right cases was noticeably greater when the second sound was the weaker of the two compared. Thus the ordinary time-error was consistently reversed. The writer cannot explain this fact.

The results as a whole offer considerable evidence for, and little or no evidence against, the ordinary belief that intensity is the main criterion in estimating the distance of a sound. The writer is not prepared to explain the divergence between the results of these experiments and the results obtained by Professor Von Kries, in support of a distance-quality or mark, but must be content to point out that the results here presented are the more numerous and that they were obtained by a method which was scarcely less precise than the method of Von Kries.[1]

[1] See Von Kries, 'Ueber das Erkennen der Schallrichtung,' *Zeit. f. Psych. u. Phys. der Sinnesorgane*, I. (1890), especially pp. 246–247.

II.

The Perception of the Distance of Sound.

BY DANIEL STARCH, PH.D.,
WITH THE ASSISTANCE OF ANNE L. CRAWFORD, B.A.

The object of this experiment was to determine the accuracy of perceiving the distance of a sound in a representative group of directions, to discover whether this accuracy varies in different directions, and to find the factors on which the perception of distance is based.

It was necessary for this purpose to employ as constant a stimulus as possible. The telephone click had been used in some preliminary experiments but it was not sufficiently uniform. A small wooden drop hammer, 3 cm. long by 2 cm. in diameter, was devised which produced a satisfactory stimulus. The handle of the hammer, 20 cm. long, was set in a pivot so that it could be swung freely up and down. The hammer was held by a catch-spring from which it could be released easily and quietly and dropped nine centimeters upon a wooden block covered with chamois. The block, the pivot of the hammer and the catch-spring were all mounted on a small bar of wood which served as a convenient handle in operating the hammer. The sound thus produced was constant and of sufficient intensity to be readily perceived.

As a guide in determining the distances of the stimuli, a narrow bracket bearing a centimeter scale and projecting toward the center of the room, was fastened to the wall at the level of the observer's head. The room was an unceiled eight-sided steeple room, two and a half meters in diameter.

The method of the experiment was as follows: The observer sat with closed eyes on a stool in the center of the room so that his ears were on level with the hammer. The stimulus was first given at the standard distance, one meter from the center of the observer's head, and then approximately two seconds later at a certain interval, say fifteen centimeters, nearer or farther. The observer then gave his judgment of 'nearer' or 'farther' comparing the second sound with the first. Two of the observers, G. and S., made after each trial a brief introspec-

tive statement of the basis of judgment. The other observers did so only occasionally. In this manner twenty-five judgments were obtained in succession for a given direction. If more than 84 per cent. were correct the next smaller interval, in this case ten centimeters, was used and if less than 68 per cent. were correct the next larger interval, twenty centimeters, was tried for the same direction. These percentages were empirically found to be the widest limits on the basis of which to calculate safely, by Fullerton and Cattell's table, the threshold of difference necessary to have 75 per cent. of the judgments correct. The series of distance intervals was three, five, ten, fifteen, . . . forty centimeters. The total number of cases for a given position in which the second sound was nearer, and of those in which it was farther, were equal, but the cases followed in irregular succession.

Thirteen directions, all in the right half of the horizontal plane, were tested: 0°f (*i. e.* straight in front), 15°rf, 30°rf, 45°rf, 60°rf, 75°rf, 90°r, 75°rb, 60°rb, 45°rb, 30°rb, 15°rb and 0°b. The stimulus hammer always remained in the same position, and, in order to test the different directions, the observers turned to the required positions. These were determined by means of Titchener's sound cage which was suspended at the center of the room. The direction were tested in succession in the double fatigue order, taking twenty-five judgments at a time for one direction. The sittings were about forty-five minutes long.

The results are presented in Tables I. and II. The figures in the tables represent in centimeters the distances which the second sound was required to be nearer or farther than the standard in order that it might be perceptibly nearer or farther. For example, the first figure in Table I. means that for G. the second sound had to be 20.8 cm., nearer or farther than the first or standard sound in order to be noticeably nearer or farther. Table I. contains the measurements obtained from G. and S., the two experienced observers. G. is associate professor of psychology and S. is the writer. The former had no acquaintance with the problem whereas the latter had planned the investigation. Each gave 100 judgments for each direction, in all 2,600

judgments. Table II. contains the data obtained from the six untrained observers, each giving 50 judgments for each direction, altogether 3,900 judgments.

TABLE I.

	0°f	15°rf	30°rf	45°rf	60°rf	75°rf	90°r	75°rb	60°rb	45°rb	30°rb	15°rb	0°b
G.	20.8	17.8	19.7	16.6	20.0	16.1	16.3	21.5	20.0	19.6	18.7	16.2	19.1
S.	11.7	12.4	11.5	14.5	10.9	15.0	9.8	12.8	9.6	11.6	12.7	9.7	8.2
Av.	16.2	15.1	15.6	15.5	15.4	15.5	13.0	17.1	14.8	15.6	15.7	12.9	13.6

TABLE II.

	0°f	15°rf	30°rf	45°rf	60°rf	75°rf	90°r	75°rb	60°rb	45°rb	30°rb	15°rb	0°b
R.	13.5	12.7	14.3	16.3	13.2	13.0	16.4	13.0	15.6	11.2	16.8	11.9	12.9
Su.	18.2	19.3	15.9	18.7	18.7	15.5	12.2	11.1	12.2	10.5	16.0	9.8	12.0
J.	7.9	11.0	6.8	7.1	8.8	3.7	3.5	2.7	3.2	5.0	3.3	5.3	2.7
K.	16.7	16.6	22.4	15.1	25.2	26.1	16.8	23.2	26.3	21.6	30.0	17.0	24.5
C.	18.8	22.4	15.5	22.4	20.9	21.4	26.9	18.0	21.3	28.9	16.3	18.4	16.0
W.	7.6	10.0	21.7	14.7	12.2	10.2	15.9	11.0	14.7	18.1	10.9	13.4	11.6
Av.	13.8	15.3	16.1	15.7	16.5	15.0	15.3	13.2	15.5	15.9	15.5	12.6	13.3

These figures give a definite answer to the questions in whose interest these experiments were made.

First, in regard to the accuracy of perceiving the distance of sound, they show that the least perceptible difference between the distances of sounds is approximately 15 cm. when the sounds are a meter away. The averages in both tables are in the neighborhood of 15 cm. The individual records agree quite closely with the exception of the unusually accurate record of J.

The second aim was to discover whether the accuracy of perceiving the distance of sound varies for different directions in the same way in which the accuracy of the perception of direction varies for different regions. The results plainly demonstrate that the accuracy of the perception of distance is the same for all the directions tested. The averages all lie within the range of 13 and 17 cm. without indicating any uniform tendency toward greater accuracy in one region than in another.

Third, in reference to the factors on which the auditory

perception of distance is based, the introspections recorded in connection with each judgment of G. and S. revealed several elements, namely, differences in intensity, in pitch, and in quality, for the different differences. The introspections accompanying each one of the 2,600 judgments of G. and S. were tabulated in order to determine the relative significance of these factors. By far the most important one is intensity. With G. 95 per cent. and with S. 92 per cent. of the judgments were said to be based wholly upon intensity. A sound was judged nearer when it seemed to be more intense, and farther when it seemed less intense than the standard. The remaining judgments were based partly or entirely upon differences in pitch and quality. But these factors did not seem to be used consistently. Sometimes the farther sound seemed higher in pitch and sometimes the nearer one. There was, however, considerable uniformity among the judgments taken at one sitting.

Visual imagery of the position of the sound was mentioned a few times and was probably only a concomitant process. The occasional introspections of the untrained observers indicated the same factors, as those mentioned by the trained observers, giving the greatest importance to intensity.

DISCUSSION.

DARWINISM AND LOGIC: A REPLY TO PROFESSOR CREIGHTON.

In his interesting paper, having the same title as this note, published in the Darwin Number, May, 1909, of this REVIEW, Professor J. E. Creighton cites my work, *Thought and Things*, as representative of the Darwinian point of view in logic, and criticises it in some detail. I am, of course, gratified that the work is honored in this way. I find, however, that Professor Creighton's criticisms are not altogether valid, and I will accordingly suggest certain considerations which in my opinion show this.

Professor Creighton has no difficulty in showing by quotations from my different publications, that I am a Darwinian, and that Darwinian conceptions have had frequent application in my work; this I am making explicit enough in a little book on *Darwin and the Humanities* now in press.[1] Nor has he greater difficulty in showing that I often take the standpoint from which experience is looked upon as an immanent self-integrating movement. But he considers these two points of view inconsistent with each other: one interprets experience 'biologically' — as a relation of organism and mind to environment — the other 'logically' or 'teleologically' (so Professor Creighton) — as a principle of internal organization and movement. The question then is this: can both of these points of view be held at once? — or does either commit us to a philosophy which excludes the other?

Evidently the first, the method and view-point of biological science, must be upheld if we are to have a theory of mental development and evolution at all. Each mind grows up in a body, and both mind and body are in environments. Experience requires things and situations: its own movement establishes and utilizes what we call the 'trans-subjective reference.' Is the recognition of this consistent with a theory which interprets experience as a progressive organization having its own 'logic'?

Professor Creighton thinks that the latter point of view commits one to a 'teleology' which — though somewhat vague to me — seems to require the denial of the validity of a Darwinian conception of

[1] Review Publishing Co., Baltimore.

adaptation, considered as a necessary factor in the development of experience.[1]

Proceeding then to the criticism of my views, made by Professor Creighton, I may say that it is in my last work alone, the 'Genetic Logic,' that I have taken exclusively the point of view of experience. It should not be compared with the other more biological books and papers except as this difference is recognized.

In the *Genetic Logic* the attempt is made to trace out the actual movement of experience from mode to mode, all of these modes being equally 'psychic.' The result is reached that a dualism of controls, due to segregation of contents, is come upon *in experience itself*. This dualism is not injected by our interpretation, nor read in from an external point of view: *it is found by and in the process*. The important point is that by its own immanental movement into the logical mode, experience *establishes just the dualism that science adopts and employs*. In the discussion of the relation of the 'psychic' and 'objective' points of view (*Thought and Things*, I., chap. II., §§ 3, 4), I show that the latter is simply the explicit outcome of the dualism normally established when the mode of judgment or reflection is reached.[3] The scientific is simply the logical point of view made use of as deliberate method. It involves the self judging or thinking and objects judged about or observed — objects known to it as 'things.' This very dualism is the presupposition of the logical as such; and scientific method — whether its results issue in Darwinism, Lamarckism, vitalism, mechanism, teleology or any other type of theory — is

[1] He uses the expression 'genetic or teleological' as if these two terms were synonymous (p. 185).

[2] It is a conscious and deliberate difference, and cannot be looked upon as a contradiction unless it can be shown that one of the points of view is rendered invalid when one takes the other. In the *Social Interpretations* both methods are used on occasion, to supplement and confirm each other, the biological however having a very subordinate place. In the *Genetic Logic*, the standpoint of experience, the 'psychic' point of view, is consistently maintained. It is erroneous, therefore, to say (Creighton, p. 180), "Professor Baldwin's account professes to show, not how the mind becomes conscious of its own logical nature, but how that logical nature is engendered in it through the motor adjustments of the organism to material conditions." How the mind becomes [grows to be] conscious of its logical nature [or processes] is just what the *Genetic Logic* does profess to show.

[3] Instead of allowing Professor Creighton's interpretation to the effect that the 'inner and outer controls' are in my hands 'a translation into other terms of the organism and environment,' I hold that the relation of organism and environment is *a logical transformation of the dualism of inner and outer controls*.

thinking, no more and no less than thinking. In the more refined operations of thought upon ideas, the ideas are symbols of the things into which they are at any time convertible. The sciences of observation go directly to the things, to perceptions and sensations; but in both cases the control of the context, whether it be one of ideas or of things, is the same — that of a sphere *taken by the process to be foreign to itself*.

So far then from finding a contradiction between the point of view of evolution — dualistic as it is — and that of a truly psychic account of the genesis of knowledge, I find that *the latter issues in and justifies the former*. Any adequate tracing out of the progression of knowledge, within experience itself, shows it to issue in a system of judgments in which the two controls — things as 'outer' and the self as 'inner' — are found confronting each other. Reflection sublimates this dualism by erecting a mediating context of ideas; but all validities in the context and all truthful references beyond it, rest upon the fact that this mediation is dual.

What then I would insist upon is the radically unreal character of the supposed contradiction. The observation, experimentation, analysis, etc., of biological science, as of all science, are processes proper and vital to the logical mode of experience. Science is logical process proceeding under its normal and necessary presuppositions. In recognizing the externality of things — the environment — it is only following the essential movement of psychic process, which although presupposing externality, still finds it to be a meaning of contrast with the internality of the inner control, of the self. Accordingly, one may freely use the biological method and point of view (as I have done in the paper on 'selective thinking' which Professor Creighton considers very reprehensible in this respect); for this procedure only recognizes as valid, for purposes of deliberate observation, the dualism that logical experience itself establishes for all the processes of thought.[1]

Of course, the further question will be asked: Is one's final philosophical view then to be dualistic? — is logical experience to be taken at its word and as the final word? Professor Creighton, as just cited, says that I recognize only two alternatives, mechanism and aprioriism; and he suggests the third, teleology. But my recognition of these two modes of interpretation is merely to cite them as horns of a

[1] It is clear then that the following statement of my view is not correct (Creighton, p. 184), "here as elsewhere the alternative for Professor Baldwin is between deriving logical principles mechanically and *finding them existing a priori*" (italics his).

dilemma both of which are to be avoided.[1] The teleological interpretation, also, taken in its ordinary sense — barring its excessive ambiguity — is also to be questioned, and for much the same reasons. These reasons I may now briefly state.[2]

1. We are only remaining true to the standpoint of experience itself in seeking to trace out the rise and development of such categories as mechanism and teleology. They arise as meanings attaching to different sorts of experience; and by them objects and situations are consistently and profitably apprehended and treated. Some experiences have a certain regularity and lawfulness: these, thus apprehended, come to mean the mechanical. In the case of other experiences, developing conation shapes the contents toward personal ends: these, so apprehended, mean the teleological. In the logical mode, these two meanings become general ways of assimilating events of one type or the other. Each is valid for its purpose, and each is restricted in its use: one means to experience just the dominance of external, the other that of internal control.

Now to use either of these as an exclusive or universal mode of interpretation is to abolish the other in its own province, and so to falsify our report of the progression of experience in which they have together arisen. The mechanical would not be mechanical but for the possession of those characters which show it to be bare of teleological meaning; it represents knowledge formed under a control which evidences itself as foreign. The teleological, on the other hand, would not be teleological but for its character as embodying the agent's control exercised in the pursuit of personal ends. Teleological processes as such are for consciousness not mechanical, and mechanical are not teleological.

I have contrasted the results of these two modes of process by using the two expressions 'knowledge through (external) control' — issuing in sequences which are mechanical in their meaning — and '(internal) control through knowledge' — issuing in sequences with which personal interest and conation are identified (*Thought and Things*, II., chap. XIV.). Unless the teleologists can show, from the movement of further experience, that there is positive justification

[1] I do not accept the term 'mechanism' as applicable to a genetic movement proper; it denotes only one of the possible naturalistic interpretations of this movement. My own interpretation, embodied in the theory of 'genetic modes,' combats the mechanical view.

[2] The following has reference also to Professor Creighton's paper read at Baltimore to which I listened. It may suggest to him some revision of that paper, since this discussion is new.

for the step,[1] they may not employ as a universal solvent the partial meaning which they favor.

2. But even if we allow the category of teleology to apply universally, it also issues in a characteristic dualism from which there is no logical escape. Ends are attained *through the mediation of ideas or facts.* Facts and ideas are not ends: 'what a man hath why doth he yet hope for?' — it is a further realization, beyond the idea or fact, that he hopes for. A conscious end is always meditated — furthered or hindered — by some fact or idea. To any teleology which involves genuine purpose, the dualism of 'fact-idea and end' — taking the form of 'means and end' or of 'hindrance to end' — is as stubborn as that of 'thinker and thing' in the domain of cognition.

To escape this difficulty, the intellectual idealist goes over to a teleology which does not involve purpose in any concrete or actual sense, while he still retains vaguely the principle of 'means and ends.' But what 'means and ends' can mean apart from an agent who adopts the means (facts or ideas) to attain the ends (results), it is difficult to see. What is really present is the actual flow of genetic process, with its great dualisms of knowledge and purpose. If we take this process for what it is, it discovers itself to experience in the two modes of organization called teleological or mechanical according as the situations of actual life present contents of one sort or the other.[2]

[1] Actually the progress of experience, both personal and racial, is away from animistic and anthropomorphic teleological interpretations of nature. Science has had gradually to achieve its birthright, only gradually establishing a conception of natural law which operates without 'teleological' interference. Just here is, in fact, in my opinion, the great service rendered by Darwinism to philosophical thought: it once for all established a natural law of adaptation.

[2] In my discussion of 'genetic series' as such (the theory of 'Genetic Modes,' *Development and Evolution*, chap. XIX., described by Professor Creighton as a sort of invalid compromise), I have pointed out that such series present both aspects, the quantitative or mechanical and the qualitative or in the large sense 'worthful': they show a form of sequence or conditioning which is not exhausted by either interpretation taken alone. Professor Creighton is, I think, in error in saying (p. 182) about this theory that 'the something new' that it recognizes as arising in a genetic series 'simply comes into the series as a miracle.' I reply: it is not a miracle except to one who has already adopted a quantitative or mechanical conception of all natural change. Such a cast-iron quantitative conception apart — why should not nature produce novelties? James and Bergson, as well as the present writer, have recently protested against the arid 'energistic' conception of 'cause and effect.' For my part, I am not willing to prejudice the case by using the terms of mechanics for such sequences; I have employed the term 'progression.' . . . Further, I do not admit Professor Creighton's claim that a genetic series, as I conceive it, in my theory of 'genetic

If this actual genetic movement, so apprehended in experience — the progressive integration of contents, as on occasion both 'factual' and 'end-fulfilling' for the agent, is what Professor Creighton means by 'teleology' — then I am with him. I prefer that term to 'mechanism,' if one is to use but one term for the entire movement. But my aim is to go further constructively, and to discover what the issue is when the movement does not stop with the *mediation by ideas* in either of these two ways — with mediation as true for knowledge, and as good for purpose — but when it goes on to apprehend the contents in a further mode of direct contemplation. The movement then goes beyond the objectification of the contents in judgments of fact and value; and reaches a higher hyper-logical immediacy.[1] My present purpose is accomplished, however, in showing how it is possible to turn the edge of Professor Creighton's criticism. I accept both the terms of the supposed contradiction. I hold that when legitimately employed both mechanism and teleology are naturalistic or empirical categories, both valid, but both restricted, in their proper use, and both superseded in a hyper-logical mode of experience.

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modes,' 'exhibits no identity throughout the different stages of the process.' On the contrary, the varying degrees of identity which it actually has for consciousness serve as motive to the transformations of the 'sameness' meaning, as traced in my book in great detail, up to the logical judgment of identity (Vol. I., chap. VIII., § 3, and chap. IX., § 5; Vol. II., chap. X.).

[1] To the development of this point much of the third volume of the *Genetic Logic* is to be devoted. In an article entitled 'Knowledge and Imagination,' PSYCHOLOGICAL REVIEW, May, 1908, I have stated in outline the characters in virtue of which æsthetic experience appears to discharge this office.